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ORIGINAL LECTURES.

CLINICAL LECTURE ON A CASE OF PRURIGO.

BY HANS HEBRA,

Privat-Docent Medical Department University of Vienna, Austria, February 7, 1882.

GENTLEMEN,—Our first patient exhibits in a very perfect manner a very interesting skin disease. You will note a variety of efflorescences over the entire body; but the greatest number of macules, papules, vesicles, pustules, and crusts are situated upon the *extensor* surfaces of the extremities. Upon the flexor surfaces the eruption is much less intense. Upon the inner surface of the elbow-joint, over the popliteal space, there is absolutely no efflorescence, and the skin is apparently healthy. Note, also, the induration, on both sides, of the lymphatic glands in the groin. Between the nipples and the knees the efflorescence exists in a very slight degree.

Now observe the general condition of our patient. She is a young woman of 15 years, but with the physical development of a child of ten. There is extremely little subcutaneous fat, as is shown by the thin folds of skin, which can be pinched up. Her muscles are slightly developed, while her bones are small and delicate.

The disease seems to be a chronic one; and, when I ask her, she tells me she has had it for ten years. She has been in our hospital a number of times.

There can be no doubt as to the diagnosis from purely objective symptoms. Just here, gentlemen, let me insist upon the importance of making your diagnosis, in the first place, purely objectively. When a patient, and particularly a lady, in private practice, commences to tell her story of her complaint, aside from the useless loss of time which careful attention entails, your mind is more or less biassed by her statements, and you do not make a perfectly cool, so to speak, post-mortem diagnosis. My rule invariably is to make a purely objective diagnosis, and then listen to the subjective symptoms.

We have before us a case of *prurigo*. At the International Medical Congress last summer the older English dermatologists denied very vehemently the existence of such a disease. Sir Erasmus Wilson was particularly emphatic in saying prurigo was only a chronic eczema. However, the younger Englishmen, all Continental authors, and most Americans admit the existence of the disease. We know very little of the disease itself. We style it an *idio-neurosis*. You see only the artificial eczema,—the effects of the disease, not the disease itself. It occurs chiefly in young people of bad constitutions and in the old age of fat people who have become thin. In the latter case it constitutes the pruritus senilis, with which you are all more or less familiar. The only difference between pruritus senilis and prurigo is that the former affects the entire surface of the body uniformly and is not so sharply confined to the extremities. The itching is universal and very distressing. The diagnosis of this affection is with us Austrians a very important matter, for reasons which I shall speak of later. The only form of artificial eczema with which it can be confounded is that of scabies. The eczema of scabies, however, affects the trunk, particularly between the nipples and the knees, while prurigo affects the extensor surfaces of the extremities.

In the Austrian army prurigo exempts the individual in whom it occurs from all service. I had a friend, a surgeon, who, in examining a man for admission into the service, did not distinguish between the two forms of artificial eczema,—that of prurigo and that of scabies. Scabies, in passing, let me say, does not disqualify a man for becoming a soldier. The man had just come out of the hospital, and he was apparently perfectly well. My friend wrote for him the usual certificate of perfect physical condition, thinking the traces of the skin disease were the results of scabies. When the soldier arrived in Bosnia he was exposed to the usual hardships of a campaign. At night the noise of scratching was insufferable to the other soldiers. While on a battle-field the poor fellow forgot to shoot, and fell to scratching. He was sent back to a hospital, and after a short interval he became well enough to return to his regiment. Soon, however, the prurigo recurred; and he was sent back to Vienna for medical inspection, and was there exempted from duty by reason of the nature of his affection. My

friend the army surgeon, however, was held responsible for all the expense incurred by the State in transporting, feeding, and clothing the man during his connection with the army. This amounted to three to four hundred florins. Now, supposing a recruiting surgeon examines six to seven hundred men daily, the risk of decimating his pay is considerable.

The prognosis, as regards ultimate cure, in prurigo, is very unfavorable. After the disease has attained any age, its radical cure is impossible, as yet. This girl's disease is about ten years old, and she will never experience permanent relief unless she remains under constant medication. If she were rich, she could take a sulphur-bath twice a week and spend her life in comfort. As she is situated, she will be properly treated in the hospital, will go out to her work apparently perfectly cured, only to return when the itching becomes again intolerable. You have heard her say she has been in the hospital already three times.

A prominent member of the Austrian diplomatic corps has been a patient of mine, suffering from *prurigo*, for years. As he keeps under constant treatment, he rarely suffers inconvenience from his disease.

ORIGINAL COMMUNICATIONS.

THE ETIOLOGY AND PATHOLOGY OF DUPUYTREN'S CONTRACTION OF THE FINGERS.

Read before the Philadelphia County Medical Society, January 11, 1882,

BY W. W. KEEN, M.D.,

Surgeon to St. Mary's Hospital, Philadelphia; Fellow of the Academy of Surgery of Philadelphia, etc.

CONTRACTIONS of the fingers may arise from very many different causes, as, *e.g.*, from vicious cicatrices following burns, from contraction of the flexor muscles, either as a primary evil or as a result of paralysis of their opponents, from diseases of the joints, from palmar abscess, from a felon, from accident, etc., or, finally, from contraction of the fibrous structures of the palm, especially the palmar fascia. This last, very commonly and also very properly named after the eminent French surgeon, is the only form I shall consider to-night. Nor can I do more than refer to its seat and its cause, for to attempt to do more would unjustly tax your time. I must, therefore, leave other considerations, and especially the treatment, to the future; while, however, I show you by these photographs the admirable results I have obtained by the subcutaneous operation.

The clinical history of such a case is very peculiar. In brief it is as follows. A man usually of forty years of age or over, generally without any assignable cause, will observe that his little or ring finger is slightly stiff. On making the attempt, he finds that complete extension is hindered in some unknown way. If a man of quick observation, he may perhaps notice in the palm two or three little, bean-like, smooth, and slightly tender nodules in the axis of the affected finger, and that the skin is a little depressed in a crescentic pit at one or two places, usually about the level of the lowest or middle transverse palmar line. Gradually, extending over several years, from four or five up to fifteen or twenty, but without any pain, the trouble increases. The little, hard nodules coalesce into what at last becomes a well-marked cord, extending to the finger (though the cord often forms without any such antecedent nodules); the skin becomes still further retracted, forming two or three very deep crescentic folds with the convexity upwards, sharply lifted by the cord, to which it is intimately adherent at its palmar edge, with deep hollows on each side of it. The finger, which at first only had its extension limited, has slowly but surely flexed, until now the first two phalanges are each, it may be, bent at a right angle, so that the finger-tip nearly touches the palm. Worse still, the immediate neighbors on one or on both sides have begun to flex, and are but little behind the first. Meantime, too, the other hand, at a varying interval of years, has probably suffered from the same deformity, so that at last one or more likely both hands are in part or wholly useless for most of the occupations of life. Nay, more, if he live to an advanced age, he may not only find the hand useless, but the finger-tips or finger-nails by further flexion may bore into the palm, producing painful ulcers, which, as in the case of a patient now under my care at eighty-five, may render life a burden.

The patient, meantime, finding the disability becoming a serious annoyance, has sought medical advice, and has used liniments, ointments, bandages, baths, elec-

tricity, massage, and, these all failing, he has gone to the surgeon, only to be told that an operation can be done by cutting the tendons of the flexor muscles, to which the trouble is due, but that, while such an operation will relieve the flexion, it will only substitute for it completely and permanently extended fingers. His last case then would be worse than the first, for, whereas permanently flexed fingers are to some degree useful, permanently extended fingers are only in the way, and subserve no purpose in life.

The first step in relieving disease or deformity is to find out precisely where the trouble lies and what is its cause; and in these two respects, even at the present day and in the case both of eminent surgeons and well-known authors, there is so much error as to this form of finger-contraction that I have thought it worth while to review the matter afresh and in the light of all the recorded facts. Up to the present time the literature of the subject has been entirely fragmentary in character. No one has ever made any systematic effort to collect and compare the recorded cases of the disease, and by analysis to obtain data upon which a just reasoning could be founded, whether as to its symptoms, its etiology, its pathology, or its treatment.

The deformity is quite a frequent one. Dupuytren says that up to 1833 he had seen thirty or forty cases of the disease; and I have myself collected the facts in twenty-three cases, of which I have seen the most within the last three years. To these I have added also one case each of Drs. Packard, Hunt, and Ashhurst of this city. Yet from almost the entire literature of the disease (for my work, though not quite completed, includes nearly every case) I have only been able to obtain ninety-five other cases. While more or less such patients have been seen by nearly every practitioner, yet generally they have been dismissed as irremediable, and nothing further has been said or published about them. Many of the records, too, are but fragmentary, stating, it may be, but the sex or hand or fingers; but so far as they went they were of value. I must also record my obligations to Dr. F. C. Sheppard and Mr. S. L. Caldwell, Jr., for the important aid they have given me in the search for cases and their tabulation.

In order intelligently to consider the pathology and etiology of the affection, however, it will be needful briefly to give the results obtained in certain particulars.

1. The *age of onset.* Many cases are too inexact for tabulation, simply saying, for example, that the disorder had existed in "an old man" for "many years;" but clearly it usually arises in middle or later life. Of forty-four cases, twenty-eight began after forty, nine between thirty and forty, and only seven before thirty. Of these seven, two certainly, and probably a third, were congenital.

2. The *sex* is noted in one hundred cases, and shows very unequal liability. Of these, ninety-one were males, and nine females. But Adams and a number of other authors say they have never seen it in women. Some even go so far as to say that it does not occur in women.*

3. The *occupations* may be classed as those involving manual labor or those exempt from it. Seventy-two cases have the occupation recorded. Of these, eighteen are manual, and fifty-four non-manual. The occupation of only two women is given. These were both non-manual, and presumably the others were also. They are not included in the seventy-two.

4. The two *hands* are attacked in nearly equal proportions. Of seventy-two cases in which it is recorded, the deformity is noted in the right hand alone twenty-three times, left hand alone thirteen times, both hands thirty-six times. The right hand was attacked therefore in all fifty-nine times, the left hand forty-nine times. The order in which the hands suffered is stated in nine cases; of these, the right was first involved four times, the left five times.

5. But, while the two hands are attacked with nearly equal frequency, the three ulnar fingers are by far the greatest sufferers. The fingers attacked are recorded in one hundred and five cases. Of these—

(1) The thumb was involved nine times.

(*a*) In five cases with all the other fingers.

(*b*) In one case with three others, including the ring.

(*c*) In one case with "several others."

(*d*) In one case with the little and ring.

(*e*) In one case with the middle and little.

* Since the above was written, several articles have appeared in the *British Medical* and other journals, on Dupuytren's Contraction, and especially as to the question of its appearance in women. I may say, therefore, that up to March 4, including those in the articles alluded to, I have added twenty-six more cases to my table, of whom eleven are women, making in all one hundred and six males and twenty females.

(2) In eight cases the index finger was contracted alone.

(*b*) In one case it was contracted with the middle and ring.

(*c*) In five cases with the middle, ring, and little.

(*d*) In one case with the middle.

(3) In four cases the middle finger alone was contracted.

(4) In thirteen cases the ring finger alone.

(5) In eight cases the little finger alone.

(6) In thirty-six cases the ring and little fingers only.

(7) In seven cases the middle and ring fingers only.

(8) In one case the middle and little fingers.

(9) In seventeen cases the middle, ring, and little fingers.

In brief, the thumb was contracted nine times, the forefinger thirteen times, the middle finger forty-five times, the ring finger eighty-eight times, the little finger seventy-seven times, the ring and little finger together sixty-five times. It is to be noted also that the bilateral cases were not always symmetrical as to the fingers which were attacked.

6. The *phalanges* also are very unequally affected. This I find recorded in fifty-seven cases with seventy-three hands.

(1) In fifteen cases the first phalanx alone was affected.

(2) In seven cases the second phalanx alone.

(3) In forty-five cases the first two phalanges were flexed, in which fifteen times the first phalanx preceded the second, and five times the second preceded the first. In twenty-five cases the order was not stated.

(4) In six cases the third phalanx also was involved.

7. The *hereditary character* of the affection is undoubted. Besides the general statements, such as Mr. Adams's that he has seen it in two brothers "several times," I find that heredity is noted in thirty-seven persons, nearly one-third of the recorded cases.

In two cases it occurred in one generation, affecting four persons.

In eight cases in two generations, affecting twenty persons.

In three cases in three generations, affecting nine persons (in one case it skipped from grandparent to grandchild), and in one case it occurred in four generations, affecting four persons.

8. The existence or non-existence of gout in the patient or his family is recorded forty-eight times. Of these, forty-two were gouty, and six were not.

9. The character of the palmar cord merits a more careful study. As it forms, the subcutaneous fat disappears and the cord and the skin coalesce, but only at certain points along the ridge formed by it. On either side of it the skin quickly becomes freer, and at a very short distance is entirely free. This disappearance of the fat, which I regard as the consequence of the pressure of the tightening cord, Madelung, a recent German writer, regards largely as a cause, since its absorption with age exposes the palmar fascia to "labor-insults," leading to chronic inflammation, thickening, and retraction. I do not find, as a matter of fact, that the fat is generally absorbed in these contraction cases. It disappears only in the lines of pressure.

The cord arises usually in the palm at about the level of the web of the thumb, or a little higher. It becomes more and more marked as we go lower down, and stretches as the chord of an arc to the fingers, where it terminates in two ways. 1. Near the base of the finger it divides into two lateral cords, which are inserted usually into the base and sides of the first phalanx, but sometimes reach the base of the second. Sometimes only one of these lateral cords is developed, and not always on the similar sides of adjacent fingers. 2. Sometimes, instead of splitting into two lateral cords, it is prolonged as a single cord in the middle line of the finger, when it generally is adherent to the skin midway down the first phalanx, and then separating from the skin divides into two lateral bands, which are attached to the sides of the base of the second phalanx. This median position of the cord was noted so long ago as 1833, by Velpeau, and has been insisted upon by Eulenburg, in 1864, as a diagnostic point in differentiating "strang-contracturen" from contractions of the palmar fascia,—a needless refinement of diagnosis, as his assertion that the thumb has no connection with the palmar fascia is erroneous. Even in Dupuytren's first operation lateral sections were not sufficient, and he was compelled to divide the median bridle also.

Not uncommonly both lateral and median cords coexist. I well remember how

I was puzzled at first to account in any way for this median cord.

Pathological Anatomy.—The disease is often misunderstood even by many of the best-informed surgeons. It is commonly supposed to be due to contraction of the tendons of the flexor muscles, and hence the opinion that their division would lead to a permanently extended finger. That this is a very likely error any one who has seen a typical case will at once admit, for so strong and stout is the cord that at first sight it would scarcely seem possible that it could be anything else. But on reflection several things would make us doubt the accuracy of this belief.

1. The well-known sheaths of the flexor tendons. These begin just above the knuckle-joints, and so firmly are the tendons bound down, both by these sheaths and the arches of the palmar fascia through which the tendons pass, that this fact alone should have aroused the suspicion that the bands were not the flexor tendons. And it is exactly at this point that the tight cords are the most elevated from the level of the bones. Mr. Adams has well insisted upon this point. He might also have added that in undoubted cases of the stoutest contraction of the flexor muscles the cords formed by the tendons are marked in the palm but very feebly elevated over the knuckles, thus showing the restraining influence of the sheaths. Not uncommonly in Dupuytren's contraction two finger-tips can grasp the skin in the interval between the elevated cord and tendon, and the movements of the latter can be perceived on flexing the finger.

2. The attachment of the flexor muscles. This is to the bases of the second and third phalanges. Hence flexion of the further phalanges should precede that of the first, which would only bend after the last two had been flexed, as in closing the fingers after paralysis of the interossei. But the analysis of my table shows that in fifteen cases the first phalanx alone was flexed, and in fifteen more its flexion preceded that of the second, and in only six cases was the third phalanx flexed at all.

3. While the cord, if it stretched to the second phalanx in the middle line of the finger, might be mistaken for the tendon, by no possibility ought it to be so mistaken if it split into two lateral cords inserted into the base and sides of the first phalanx, for the tendon does not so divide, nor does it have any attachment whatever to the first phalanx.

4. Again, in true contraction of the flexors, as in the secondary contractures of hemiplegia, the tendons behave very differently on the application of continued forcible extension. Usually in a short time the fingers will yield to a greater or less degree, and flexion of the wrist will allow of their immediate extension. But in Dupuytren's contraction flexion of the wrist has absolutely no influence on the condition of the fingers, and Dupuytren has given the best illustration of the strength of the contraction, for he says they will not yield even if a weight of one hundred and fifty pounds be attached to the fingers.

But we have better evidence than merely negative. Unfortunately, I have had no opportunity myself to dissect any hand the subject of this deformity; but I have found recorded twelve dissections, involving sixteen hands. Of these the most important and exact are those by Dupuytren, Goyrand, Sevestre, Richer, Partridge, Adams, and the St. Bartholomew's Hospital specimen. Not in a single case is any mention made of contraction of the flexor tendons, and in ten hands it is expressly stated that they were entirely normal.

It will be well, perhaps, here to quote briefly three of the dissections,—those of Richer, Sevestre, and Goyrand,—in order that we may obtain a clear idea of these abnormal bands. In Richer's case the first and second phalanges of the ring-finger were chiefly affected. The middle fibres from the palmar cord went to the skin of the first phalanx; the lateral fibres went to the sides of the first phalanx and were lost in the extensor and interosseous tendons; a feebler fasciculus extended to the second phalanx, terminating fan-like in the fibro-fatty tissue; adhesion between the fascia and the flexor sheaths was very pronounced, but the tendons were free. The fibres of Gerdy were hypertrophied on each side of the finger.

In Sevestre's case of similar character the lateral bands from the palmar fascia furnished but few fibres to the base of the first phalanx. Most of them passed directly to the sides of the second phalanx, especially at its base, and were reinforced by independent fibres, arising partly from the bone and partly from the flexor sheath.

Goyrand's case proves, according to

that author, that the cords are not due at all to the palmar fascia, but to fasciculi of wholly new formation. The bands, he states, went from the fascia to the sheaths of the tendons, the anterior face of the first phalanx and the base of the second phalanx; another to the side of the first phalanx and base of the second; another arched from the cord to the left ring finger over to the middle of the first phalanx of the middle finger, where it was attached to the flexor sheath and was prolonged to the base of the second. The thumb had two fasciculi,—one from the palmar fascia, the other from the base of the forefinger.

The explanation of these dissections we shall see when we come to study the palmar fascia itself.

Baum has attributed the cords to hyperplasia of the skin, but in the dissections above referred to the fingers did not yield when the skin was removed. Moreover, when dissected, the skin resumed its ordinary length, and Goyrand and Richer especially state that the skin was not thickened either to the eye or in Richer's case microscopically. Moreover, the skin can always be differentiated from the band, even to some extent where they are fused, and where not fused the cord is felt distinctly at a deeper level. In the early stages of the disease, also, the skin is evidently drawn down into little pits by a something below it, and inserted into it, as is well seen in these photographs of the early stage of the disease.

Malgaigne has drawn attention to the flexion which occurs in plasterers, farmers, and others of similar occupation, from the inability to extend the fingers in consequence of the hardened and contracted state of the skin. Such persons should be put in another separate category, for the conditions are not analogous. In these cases all the fingers are flexed, and the entire palmar skin is affected, with no such cords and crescentic furrows as are characteristic of Dupuytren's contraction.

Though well known for a long time previously, it was reserved for Dupuytren, in 1831, to proclaim the important fact, founded on the dissection of a case, that these contractions are due chiefly to contractions of the palmar fascia and not to the tendons. In his "Leçons orales" in 1832, and in his lectures in 1833, he repeated it again and again, and so emphasized it that he was really the first who enlightened the profession on the subject. True, Henry Cline, at least as early as 1808, in his lectures, alluded to its real character; Sir Astley Cooper also had done the same, and even had advocated the subcutaneous method of operating; but nobody paid any attention to them. Dupuytren first compelled a hearing. How emphatically he impressed his views on the profession is shown in the number of theses and other papers that soon followed, and his "marchand de vin en gros" and his "cocher d'environ quarante ans" reappear as familiar friends in nearly every one. In 1835, Goyrand presented two dissections and a valuable paper to the Académie de Médecine. He contested Dupuytren's opinion, and asserted that the contraction was not due to the palmar fascia, but to abnormal fibrous cords of entirely new formation, partly from the fascia to the fingers and partly from one phalanx to another. In reporting on the paper and specimens, Sanson expresses his belief that it is due both to the fascia and the abnormal cords, but that the latter are not of entirely new formation, but are merely due to hypertrophy of the normal fibres extending from the fascia to the fingers, which exist to a greater or less degree in every hand.

In order to judge fairly which of these views is correct, we must examine, therefore, the normal anatomy of the palmar fascia, which in several respects is usually misdescribed and misdrawn. Time will allow me only to call attention to a few points, and I must reserve details for the future.

1. Under the skin, between it and the fascia, lies a very thick layer of fibro-fatty tissue which is continued on to the fingers. This is traversed by a large number of slender but strong fibres running through it from the fascia to the skin. It is largely due to this layer that the anatomy of the palm has been misunderstood, for it is very difficult to dissect away the fat, especially on the palmar surface of the fingers, without at the same time destroying all or nearly all the fibres. The fat as it were makes a hernia in many places through the meshes of the fibrous tissue. Here readily come in what Malgaigne has happily called the "complaisances du scalpel," each individual dissector making

his results tally with his wishes. These cutaneous insertions of the fibres exist more or less all over the palm, and can only be appreciated during the dissection by their division. They cannot well be represented in a drawing or shown in a dissected specimen. They exist, I think, more strongly on the ulnar half of the fascia, and sometimes even a strong band will be found in the line of the ring finger. On the fingers they are most marked, I think, over the middle of the first phalanx. Both of these places are the very points, it will be observed, where the contracted bands and the skin are fused; and in the palm it is the point at which the contraction first shows itself by the cutaneous pits and folds.

2. The four digital tongues of the fascia are generally represented as splitting into two lateral fibrous bands which go to the base of the first phalanges and the anterior ligaments of the knuckle-joints. This is true, but is a misleading half-truth. Some fibres go to the base of the first phalanx and pass even to the extensor tendon, while others go to its sides, and, joining some of the "fibres of Gerdy," some reach the second phalanx, and possibly some even the third.

These lateral fibres hypertrophied and contracted will account for the cases of flexion of the first and sometimes those of the second phalanges by lateral bands, but they will not account for the median bands. These are due, I believe, chiefly to the median fibres derived from three sources. 1. The fibres already described, which are inserted into the skin of the first phalanx. 2. The fibres extending loosely to the sheaths of the flexor tendons. At the archway formed by the splitting of the digital tongues of the fascia for the flexor tendons, there is no solution of continuity, no archway separate and distinct from the structures under it, but a continuity of structure from the fascia to the flexor sheaths. 3. The fibres of Gerdy, as the French call them. Of these, a very insufficient and cursory description is given, and in not a few anatomies they are scarcely even mentioned. Where the fascia divides into its four digital slips there is a strong transverse band of the fascia proper, extending from its radial to its ulnar border. But at the web of the fingers, nearly an inch farther down than these, is another even stouter transverse band, first described by Gerdy. Its fibres give form and strength to the web of the fingers, and at the same time support the whole hand crosswise. It often lies very low down on the fingers, and so unites them that even in health flexion of one finger carries its neighbors with it. Not seldom in Dupuytren's contraction this direct effect is seen without any shortening of these fibres. Some of the fibres of Gerdy pass entirely across the hand, others only part-way, while others arch over from one finger to another. These last pass down partly on the side joining with the lateral digital fibres from the palmar fascia, while others pass directly down the middle line of the first phalanx chiefly in the subcutaneous tissue, and on reaching the base of the second phalanx they split astride the flexor sheath and are inserted into the sides of the base of the second phalanx. These three sets of fibres I have just described, partly arising from the palmar fascia and

Dissection of finger contraction, affecting middle and ring fingers; from specimen in St. Bartholomew's Hospital Museum. *a*. Contracted band of palmar fascia stretching across like string of a bow. *b*. Flexor tendons lying deeply along the concavity of the curve, close to the bones, and bound down along the first phalanges of the fingers by the dense tubular sheath, *c*, through which they pass. *d*. Digital prolongations extending to articulation between first and second phalanges in each finger. *Adams*.

partly discrete from it, yet analogous to it and at points fusing with it, will readily account not only for the median bands

but for the loop-like termination of them at the base of the second phalanx so well shown in the St. Bartholomew's Hospital specimen. So far as I have observed, these median bands always go to the second phalanx, causing either its independent flexion, or that of the first phalanx together with it.

Whether these abnormal bands are due merely to hypertrophy and contraction of normal fibres, or whether they are wholly of new formation, I do not pretend to say; nor do I think we can tell. And, after all, it is a piece of casuistry. That the fine and slightly-developed fibres are there, dissection will easily show; and the only microscopic examination we have — that by Richer—shows that the abnormal cords were formed of ordinary normal but exaggerated fibrous tissue. The elastic fibres perhaps were a little more numerous than usual.

The careful dissections I have alluded to confirm the opinions I have expressed. All of these tissues, whether the palmar fascia, or its prolongations on to the fingers, or the fibres of Gerdy, are fibrous in character. It is not surprising that each of them at times, at times all together, sometimes that and sometimes this set of fibres, in one case the median, in another the lateral, should be affected. Alike in texture, they suffer alike from any similar cause.

Another not uncommon statement as to the palmar fascia, to which Malgaigne gives his support, is that the thumb has no connection with it. And yet, as we have seen, the thumb is sometimes contracted. But careful dissection will show that there are two constant and (though not very stout) yet very strong bands running from the palmar fascia to the thumb, as is well shown in these dissections and drawings. One of them consists of longitudinal fibres from the upper part of the fascia, and is equivalent to a fifth digital tongue. The other comes from near the base of the forefinger, and consists chiefly of the proper transverse fibres of the fascia prolonged to the thumb. These two bands unite to a considerable extent into a single cord, which again splits and is inserted on each side of the tendon of the long flexor at the base of the first phalanx. The contraction of these constant bands is quite sufficient to account for the flexion of the thumb, and in Goyrand's case they are especially described from the dissection.

Etiology.—In considering the etiology of the deformity, two opposing views have to be considered. 1. That it is the result of a local cause. 2. That it is the result of a constitutional vice.

Following the lead of Sir Astley Cooper and Dupuytren, nearly all of the earlier writers—Goyrand, Sanson, R. Adams, and others—attribute it to the continuous or oft-repeated slight traumatisms or insults incident to occupation, and followed by slight chronic inflammation; for instance, the use of the whip, the oar, the gun, the graver's tool, the axe, the spade, the sword, the cane. Many also of the more recent writers have taken the same view, such as Post, Roque, Fort, Gerdy, Jobert, Schultz, and Baum. But many cases occurred in which neither the occupation nor the clinical history could be reconciled with the theory of such labor-traumatisms and any consequent inflammation. Accordingly, a constitutional cause, and especially gout and rheumatism, began to be suspected. So early as 1832, Avignon de Morlac, in a Paris thesis, refers to such a theory, but only to condemn it, as did nearly all his contemporaries. Guérin, in 1843, names it as probable. Elliott, of Dublin, in 1861, in an excellent little brochure, affirms his disbelief in a traumatic origin, and in the same year Menjaud, in another Paris thesis, boldly takes the ground, even in his title, that it is intimately related to gout and gouty rheumatism, and gives a remarkable series of four cases occurring in a single family. Among later writers Bulley, Madelung, Richer, William Adams, are quite decided in this view.

A third class, including Langenbeck, Pitha, Erichsen, Fergusson, Little, and Ashhurst, with variations in detail, attribute it, in general, to an inflammatory exciting cause, the result of mechanical injury, but yet recognize the fact that it often arises from constitutional conditions, among which the gouty diathesis holds a prominent place. Bryant, however, while recognizing a constitutional origin, doubts that it is gouty.

The theory of Eulenburg, that it is due to such general causes as digestive troubles, hemorrhoids, etc., or that it is due to spirits or syphilis, is scarcely worthy of a moment's thought.

Personally, after a careful consideration of the facts, I would state it thus, that while a few cases may possibly arise from injury, yet

the vast majority are due to a gouty predisposition, or some similar constitutional vice. I have personal knowledge of twenty-three out of the one hundred and twenty-one cases in my table, and of fourteen I have detailed histories more or less full. One had neither gout in his own person nor in his family, while seventeen had a gouty personal history and usually a family history of the same character. In five nothing is stated as to this point, but, as some of them were close relations of others who were gouty, it is probable that they, too, had the predisposition, if not the actual disease. Only three alleged any traumatism, and two of them were also very gouty. The large majority of all the cases I have seen had enlarged and tender finger-joints, the seats of more than suspicious pains.

One gentleman on whom I operated had a very severe and protracted attack of lumbago two days later, and another a year after. In another case, a merchant of eighty-five, in whom the disease came on at about forty-five in the right little finger and never extended any farther, this finger-contraction was the only apparent gouty trouble until he was over seventy. Atrophy of one optic nerve then set in, followed at eighty by a fierce gouty iritis, which destroyed his vision and was accompanied by an entirely disabling rheumatism of all the right arm. This and an equally rebellious eczema have persisted for nearly five years. A similar attack of gout followed two of Mr. Adams's operations, though at a longer interval, and one of them had had a gouty inflammation of the eye. How strikingly characteristic this is of gouty patients, in whom an accident or an operation is so apt suddenly to disclose the latent disease! Sir James Paget, in his charming Clinical Lectures, mentions one case in which gout attacked the hand five minutes after a fracture of the forearm, and another in which, at seventy-five, after amputation of a little finger, the patient had his first attack of gout, a luxury his family had long enjoyed.

But, however striking may be the personal experience of any one man, it is possible that it may be misleading, from its not representing the average conditions. Accordingly, I have analyzed my tables, and I think this will confirm the conclusion already formulated. The direct statement as to the presence or absence of a gouty or rheumatic personal or family history is made only in forty-eight cases: forty-two of these were gouty, and six were not,—a very striking disproportion. Moreover, in many that I could not class as gouty there were suspicious pains which were very likely of this character. It must not be forgotten, however, that most of these cases so classed as gouty are reported by later writers. Had the earlier surgeons inquired more carefully into the history and recorded the result, we should have been more certain of our ground. But, enthralled by the belief in a traumatic cause, apparently no further inquiry was made, or at least recorded, either as to the personal or the family history in this respect.

Another feature that is a strong argument in favor of its constitutional and probably gouty origin is the striking tendency to heredity. Gout has been long known as apt to be hereditary. So true is it that Paget, in the Lectures already alluded to, says it would be rare for one member of a gouty family to live to old age and not to suffer from it, if not during the calm of good health, yet in the troubles that follow injury or illness. Thirty-seven, or nearly one-third of all the cases, had the hereditary element pronounced extending even to the third and the fourth generation. In several of these cases also the occupation was distinctly such as to involve manual labor, such as a quarryman and a wood-carver. So long as the patient alone was had in view, the origin of the disease from a local cause would be extremely probable; but a larger view, embracing the family history as well as the personal, shows the greater probability to be on the side of the constitutional origin of the affection.

But other features in the clinical history of the disease render a local and especially a traumatic cause a very unlikely one.

1. The age of onset is usually middle or later life; yet, as the period of vigorous labor usually begins at or even before twenty, we would naturally look for it to arise much earlier than after fifteen or twenty years of such labor.

2. Again, its appearance in women is an argument against the traumatic origin, for their occupations are not such as to expose them, as a rule, to such labor-insults. On the other hand, their comparative immunity from gout is noticeably in accord with the statistical results we have obtained. Gar-

rod quotes the French table, giving eighty cases of gout in men to two in women, and says that it is confirmed by his own experience, and we have ninety-one cases of Dupuytren's contraction in men to nine in women.

3. While occupation or a personal habit is usually assigned as the cause of the supposed frequent traumatisms, my investigations show that in most cases of this disease the patients were not engaged in occupations or habits involving manual labor. Most, if not all, of the women were probably thus exempt; but without including them it is as eighteen to fifty-four in favor of the non-traumatic origin. Among those occupations classed as manual were the farm-laborer, wood-carver, mason, soldier, clerk (who ascribed it to the letter-stamp in the pre-envelope days), pilot, dyer, upholsterer, gardener, grocer, quarryman, coachman, paper-hanger, and mineralogist; while among the non-manual occupations were the apothecary, pianist, violinist, government official, army officer, clerk, lawyer, doctor, nurse, hospital steward, master-builder, banker, clergyman, college and railway president, merchant, and gentleman, and men who indulge in the luxury of no profession or occupation. Of course it is possible that some of these may have used their hands so as to induce injury, as a doctor who rode horseback a great deal, or a lawyer who obtained exercise by rowing; but any error from this cause would be offset by the want of such injury in some of those classed as of manual occupation. While a violinist's labor is etymologically manual, yet it is not such as to involve any labor-traumatisms.

4. In laborious occupations, as a rule, the thumb, the fore and the middle fingers also would generally bear the brunt of the work; yet we find the vast preponderance of the contractions in the ring and little fingers, and this irrespective of the character of the occupation.

5. So, too, the invasion of the left hand in almost equal frequency with the right, and in not a few cases the left hand alone, would militate against the theory of a traumatic cause. A disease which appears indifferently on either side, or more frequently on both, is far more apt to be due to a general constitutional cause than to any traumatism. Left-handedness might account for the contraction on that side were the disease very rarely sinistral in its manifestations; but its so frequent occurrence puts such an explanation out of the question.

6. It is also occasionally congenital. Dupuytren records two such cases, a child of six and its grandmother (a not insignificant element of heredity coming into play), and Gerdy records one of three years old in which it is more than probable that it was congenital. In such cases labor-traumatisms are of necessity excluded.

7. As to the chronic inflammation supposed to follow the repeated slight injuries, I can only say that absolutely no perceptible evidences of it exist as a rule. One of my cases has been under my observation for six years, and another for over thirty, yet at no time has there been the slightest evidence of swelling, heat, redness, or other inflammatory symptoms (occasional slight pain alone excepted), nor have I seen it in any other case.

8. Finally, gout and rheumatism, as we know, attack occasionally the plantar, the lumbar, and the ante-brachial fasciæ, attacks which are followed sometimes by similar obstinate and permanent contractions.

The conclusion would therefore seem forced upon us that its cause lies deeper than any local influence, and that a constitutional vice like gout or rheumatism, if sought for, will nearly always be found.

EXPERIMENTAL CONTRIBUTIONS TO THE HISTOLOGY OF BRIGHT'S DISEASE.*

BY LOUIS D. BROSE, M.D., Ph.D.,
Resident at German Hospital.

A.—EXPERIMENTS.

MY object in undertaking these experiments was to study the lesions of nephritis in their early stages, a privilege which in human pathology has been limited to a few,—which accounts for the many diversities and diametrically opposite views held on the subject. I will here credit Dr. H. F. Formad with having brought this subject to my mind as an inviting field for experimental work, and also acknowledge that I used his opinion on doubtful points relating to the microscopical appearances of some of the sections. (The work was

* Abstract of a thesis presented to the Medical Faculty of the University of Pennsylvania, March, 1881.

done in the Pathological Laboratory of the University of Pennsylvania.)

The method of procedure was to inject cantharides, from one-half to two and a half drachms, with a hypodermic syringe, and then kill the animals at different dates and compare their kidneys with those of normal animals. In some cases the kidney was cut down upon and injected directly; in other instances the drug was thrown merely into the abdominal cavity. The only other similar experiments that I have found recorded are those of Dr. Thadäus Browicz, Docent at the University of Cracow, and those of V. Cornil, of Paris, whose results I will give farther on.

I will generalize my experiments into three series, according to the seat of the lesions produced. In the first series I will include only those experiments in which the lesions were confined solely to the renal epithelium. This condition of parenchymatous change in the kidneys, as a rule, followed those experiments in which the cantharides was merely injected into the abdominal cavity. *Macroscopically* these organs presented the following appearances: *size*, apparently normal; *stellate* veins ramifying over the cortex, congested, in some cases very much distended with blood and standing out quite prominently. *Capsule* not thickened, without adhesions, and readily detached from the cortex. On section, presented a darkened and congested appearance, with the blood oozing freely from the divided structures. In some cases the dark color was interspersed with traces of yellow lines. No great prominence of the Malpighian tufts could be discerned by the naked eye. *Microscopically*, the cells were found to have taken the carmine staining but lightly, were much swollen, highly granular, and in many instances the tubules had by desquamation been deprived altogether of their secreting element. The cells lining the convoluted tubes were often found infiltrated and distended with fat. Many of the tubules that had been transversely cut across showed their lumen occluded by the swollen cells, and in some cases granular cylinders, the result apparently of disintegrated epithelial cells, could be seen filling their interior. In one case the Malpighian tufts were retracted, leaving an interspace occupied by a granular material. The walls of the arterioles were either normal or but slightly swollen: however, in one case, even minute capillary hemorrhages were found. The lesions were always most marked in that portion of the kidney occupied by the convoluted and looped tubes, the so-called labyrinth, and the straight collecting tubes either remained open or had their epithelial lining changed to a much less degree.

In the *second series* I will include those experiments in which both a tubal and an interstitial lesion resulted. These changes occurred if the irritant was injected directly into the kidney. *Macroscopical appearances:* size in most cases somewhat increased; in no instance did the organs appear smaller than normal. Capsule thickened, rough, but, as a rule, readily detached from the cortex. On section, the demarcation between the cortical and medullary zones quite distinct. While the medullary region was of a dark congested hue, the cortex appeared also swollen, but of a yellowish-gray color. The Malpighian glomeruli appeared in some cases as minute red congested points.

Microscopically the epithelial element is swollen and their nuclei much obscured by their highly granular condition. The lumen of the tubules, in consequence of this swollen epithelium, is much encroached upon, and is in some cases altogether occluded. Granular plugs may be seen sticking in the transversely divided tubes. The looped tubules and the collecting tubes of the first order contain epithelial cells infiltrated with fat; this in some places has been either reabsorbed or carried off by the urine, and there remains as a result a granular débris, which has collected in some places about the Malpighian bodies to such an extent as to push them to one side and distend their capsules. In the intertubular spaces are seen embryonic cells of a round or spindle-shape character. There is not only an increase of nuclei around the Malpighian bodies, but their capsules are also thickened, and in the tufts themselves the nuclear proliferation has taken place, giving rise to a true "glomerulo-nephritis." The walls of the blood-vessels are thickened, and around their adventitia the cell-proliferation has also taken place. Minute extravasations of blood are seen in various parts of the specimens, and this has even gone so far as to produce hemorrhagic infarcts of considerable size.

In the *third series* I will include two experiments, which I shall describe sepa-

rately. In the one case a small black dog was anæsthetized, an incision made in the left lumbar region, and the kidney injected directly with half a drachm of the tincture of cantharides. Closing the wound by suture, the animal in the course of eight hours recovered from the effects of the operation without having manifested any bad symptoms. Six days later the animal was again chloroformed, and killed by section of the jugulars. Reopening my former incision, which had by this time almost completely united, I came upon the injected kidney, in which I found an abscess situated beneath the capsule, around the point of insertion of the needle. *Macroscopical appearances of the kidney:* size increased, capsule much thickened, firmly adherent, so that in its removal strips of the cortex were also torn off. On section the cortex was found to be much swollen, of a dark congested appearance, with blood oozing freely from the divided tissues.

Microscopically, in the intertubular spaces a marked small cellular infiltration was found, many of the cells being spindle-shaped. This increase of interstitial tissue was most marked in the region of the cortex bordering on the medullary zone. The epithelium lining the tubules was highly granular, and swollen to such an extent that in many instances their lumen was occluded. On transverse section cylinders were seen in some of the tubes, in others there was simply a granular débris, while in still others even these remains of degenerated cells were absent and the tubes were quite empty. In the region of the abscess were seen many wandered-in cells; the tubules had been overrun, as it were, and were scarcely visible. Still farther from the abscess the tubules were seen intact, and the wandered-in cells simply occupied the intertubular spaces. Limiting the abscess was observed an attempted wall-formation by a reactive inflammation. The walls of the blood-vessels were much thickened, and around their adventitia was observed a proliferation of the connective tissue. *Right kidney:* size slightly increased, capsule thickened, but much less so than that of the left organ, and it was readily detached from the cortex. On section, the tissues presented the same venous congestion as in the left kidney, but to a much less degree. *Microscopically* the blood-vessel walls were found thickened, and small hemorrhagic infarcts seen in the region of the vasa recta and vasa afferentia. The capsules of the glomeruli were thickened and an increase of nuclei was observed in the intertubular spaces. The epithelium here, like that in the left kidney, was granular and swollen.

In the other experiment included under this series a black rat-dog was injected in the left lumbar region with a drachm of cantharides, the injection being made directly into the peritoneal cavity. Two hours after the injection the animal appeared quite sick from the effects of the drug. Eleven days later the dog was again injected upon the left side with a drachm of the drug. Twenty-one days from the date of my first injection chloroform was administered, and the animal killed by section of the jugulars. Making an incision through the left lumbar region, I at once came upon the kidney, which I found much enlarged. I now found that the ureter was also distended by a fluid resembling urine, and about four inches from the kidney I came upon an inflammatory stricture, the result no doubt of my injection, the needle in all probability having perforated the ureter. Severing the ureter above the point of stricture, a large amount of healthy urine escaped from the distended kidney, and its size decreased to about one-half, so that it now felt soft and flabby. Following the divided ureter down to the bladder, I found this latter viscus partly filled with urine, the secretion doubtless of the right kidney, as the stricture was impermeable to the passage of urine. Examination of the urine disclosed the presence of albumen and tube-casts chiefly of a granular and fatty character.

Macroscopical appearances: capsule attenuated and readily removed. On the surface of the cortex were seen round whitish bodies, which I took to be distended Malpighian tufts. Laying the organ open, I found the cortex much swollen and of a light-gray color. The medullary zone was of a darker hue and contained much venous blood. The hilus was much dilated, resembling a large funnel, its mucous membrane being thickened and hyperæmic. Prolongations from the dilated hilus could be traced inward as far as the periphery of the cortex. The ureter was also very much dilated, and readily permitted the introduction of my little finger. *Macroscopical* appearances of *right* kidney: size slightly increased, capsule normal; on section, the cortex was of a yellowish hue,

while the medullary portion was dark and congested. *Microscopical* appearances of both were similar, save that the lesions were greater in the left kidney. Many of the tubules contained cylinders, mostly of a granular character. The epithelium was swollen, highly granular, and infiltrated with fat. The interstitial tissue was increased, and many embryonic cells were seen not only in the intertubular spaces, but also around the thickened blood-vessel walls and Malpighian tufts. Many of the latter were wholly obscured by a granular débris, which compressed them in one corner of their capsules.

DEDUCTIONS FROM EXPERIMENTS.

The unicity of Bright's disease has lately again been strongly propagated by Professors Bamberger and Rosenstein, who conclude that the lesions always involve both the parenchyma and interstitial elements from the commencement, and that the issue into the large white kidney and the granular red one depends upon the fact that in the former the lesions are more pronounced in the secreting parenchyma, while in the latter it is the intertubular connective tissue which is most involved. I must differ from this theory, as we may have a parenchymatous nephritis, at least in its early stage, without sufficient lesion of the interstitial tissue to be recognized by the microscope; and in proof of this statement I refer to my experiments included under Series First. My own explanation as to why we have the two forms of kidney, the large white one and the small red one, is that in the former instance we have an acute process brought about by the accumulation of a *materies morbi* in the blood, such as occurs in the acute exanthematous fevers and also in the sudden chilling of the body, in which the great strain is first made upon the blood-vessels, resulting in acute hyperæmia and inflammation. The epithelial elements of the kidney react to the irritant in a similar manner as all mucous membranes, in the production of an acute catarrh. In further proof of this, I will refer to the urine of acute parenchymatous nephritis, in which blood, blood-casts, and epithelial casts are found. Now as to an explanation how we may have simply a catarrh without a discernible lesion of the interstitial tissue. This may be brought about in two ways. First, the irritation may be transient and very acute, as in my experiments in Series First, acting suddenly upon the epithelium, producing a cloudy swelling, the irritant being quickly eliminated, and, the animal possessing good recuperating powers from a catarrh, the epithelial cells may again regain their normality. This very thing we see demonstrated in daily practice, that in one person an acute catarrh gets well soon, while in another the process lingers and finally it eventuates into a wasting disease and death ensues. Secondly, owing to the great production of epithelial cells the intertubular tissue may be retarded in its growth, although this must be rare, and again we would have resulting a nephritis with hardly a discernible lesion of the interstitial connective tissue.

In case of the contracted kidney we have the result brought about by a slower process. The blood being surcharged with lithates, as in the gouty diathesis, or with lead in chronic lead-poisoning, we have the serum acting upon the epithelial cells in the region of the convoluted tubes. The cells have double work in separating the solids from the watery fluid, the process of filtration is of necessity slower and continually goes on, rendering micturition frequent. The intertubular tissue is constantly bathed in a fluid holding an irritant, the blood-vessels must withstand the renewed action of the heart in forcing the filtration, and in consequence of this slowly changing nutritive pabulum bathing the interstitial tissue, irritating it to a hypernutrition, we finally have an overgrowth of its elements similar to that which we see in passive hyperæmias elsewhere. The epithelial cells being so much more slowly irritated do not take so active a part as in case of acute nephritis, and consequently, while they may be involved, they do not show an active proliferation.

It has been denied by Dr. George Johnson ("Lectures on Bright's Disease") that we have an increase of connective tissue between the tubules, he claiming that this is only a mistaken idea of the increase of connective tissue and is based upon the thickening of the basement membrane of the uriniferous tubules, and that this, together with the wide space between the tubes occupied by atrophied remains of shrunken tubes and intertubular capillaries, has given rise to this doctrine of a new formation of connective tissue be-

tween the tubes. In this statement I must beg to differ with him. For while I can well conceive how the kidney, like any other organ in the body, is liable to become atrophied by age and show a relative thickening of the basement membrane, still we do have a true increase of the interstitial element, and this is well illustrated in many of my specimens included under Series Second. He also apparently doubts the new formation of connective tissue around the Malpighian tufts, and thinks it may be mistaken for a simple thickening of the fibrous capsules themselves. I can agree with him in that we have a thickening of the capsules, but think he is in the wrong for limiting this thickening to the capsules alone, for my preparations distinctly show that we also have a new-formed connective tissue outside of this. (See experiments under Series Second.)

My experiments gave also a different result from those of Browicz,* probably due to the fact that his use of cantharides was limited to subcutaneous injections, while my own were made either into the kidney itself, in which case there resulted the formation of an abscess, the "surgical kidney" of some authors, "suppurative nephritis" of others, and the production of marked interstitial change, or injections were made into the abdominal cavity, in which case various results followed. The most marked lesions, however, I always observed in the labyrinth, the epithelium being either cloudy, granular, or fatty. This last lesion Dr. Browicz does not seem to have observed. Dr. Dickinson ("Pathology and Treatment of Albuminuria") says that though fatty change is rapidly produced it is never seen in the very early stages of the disease. This high authority's opinion would seem to be certainly disproved by my own experiments, for I not only found this fatty condition present in a number of experiments, but also found a fatty condition in one case in an animal killed within twenty-four hours after the injection of cantharides. This lesion, then, which is described by authors as constituting the second stage of the parenchymatous nephritis, may be observed along with the cloudy swelling in the very early stage of the disease produced experimentally. To what is this fatty degeneration to be attributed? It certainly was not a post-mortem change, for I was very careful of this, and in no instance did I permit an organ to remain exposed after the death of the animal longer than half an hour, when it was put into appropriate hardening fluid.

Dr. Johnson, in his "Lectures on Bright's Disease," offers the explanation that the swollen tubules so compress the intertubular capillaries, and thus impede the circulation, that the nutrition of the tubes is impaired, and as a consequence their cells undergo a fatty degeneration. Dickinson thinks it is not the primary change, but the result of an altered nutrition of the cells consequent upon the inflammatory state. Johnson's explanation of its being due to a retrogressive change of necrobiosis I think highly satisfactory in chronic lesions; but in the early stage of the acute disease I think it more probably due to the action of the irritant in the blood, irritating the cells directly while they are excreting it, thus causing a hypernutrition rather than a necrobiosis. Therefore I will, with Dickinson, assume that it is the result of altered nutrition consequent upon the inflammatory state.

The desquamation is, I think, probably to be explained by the fact that in the cloudy swelling of the cells we have a loosening of their connections with the basement membrane, and the cells, having become also granular, resist the filtering through of the watery fluid. I would suggest, therefore, that desquamation occurs in consequence of this loose connection of the epithelium and its impermeability, in consequence of which it is necessarily forced forward into the lumen of the tubules and carried off by the urine.

The casts which I found in the urine of the animals experimented upon were of a granular, hyaline, and oily character.

The results of an experimental research by Dr. V. Cornil, of Paris,† which were published after I had written my essay, coincide better with my own experiments than those of Dr. Browicz. He says that acute cantharidin-poisoning may be compared to that which results from a single dose, and the subacute poisoning to that which follows the absorption of repeated doses.

"In the rabbit, from forty minutes to an hour after the subcutaneous injection

* Centralblatt für die Medicinischen Wissenschaften. Berlin, March 1, 1879.

† London Practitioner of August and October, 1881.

the lesions of the cortical portion have attained their greatest intensity. We observe a liquid extravasation between Bowman's capsule first; this coagulates, forming a reticulum containing in its meshes leucocytes and red blood-corpuscles, which have evidently come from the blood contained in the vascular tufts of the glomeruli. These cells finally eventuate into a granular débris. In one and a half to two hours do we first observe a change in the flat cells of the capsule itself. These now imbibe the liquid containing this granular material, swell, and many are detached by becoming spherical, and fall into the cavity of the glomerulus. We also notice a granular and fatty degeneration of the cells lining the convoluted tubes. It is not until a little while afterwards that the inflammation shows itself in the straight and collecting tubes by a multiplication of the cells and modification of their form, and by immigration of leucocytes. We have elaborated from the renal epithelium clear granular globules whose chemical nature we do not know. One sees in the middle of the protoplasm of the cells, or rather as an internal part of them, a clear cavity which projects into the lumen of the tubes. This clear globule, or, better, that part of the cell itself which constitutes the clear globule, escapes inside of the tube and is converted into a bubble.

"In the dog I have seen in one, two, or eight days, white corpuscles and a granular exudate extravasated into the capsules of the Malpighian glomeruli, just as in the kidney of the rabbit, with the exception that the white corpuscles were in less quantity than in the rabbit. The experiments upon the dog, on account of their renal epithelium normally containing more fat, and because of the abscesses following the subcutaneous injections, were not so satisfactory as those in the rabbit.

"The use of the term desquamative nephritis is justifiable only in the straight tubes. The lesions of the connective tissue, as in most cases of albuminous nephritis, are of minor importance. In acute nephritis we find disposed along the arteries of the glomeruli, and especially along the capillary vessels of the pyramids, leucocytes. If we admitted an absolute duality between interstitial nephritis and parenchymatous, we should have some grounds for saying that there was here a certain degree of interstitial involvement. But we have to do with an inflammation in which the increased tension of the blood in the vessels leads to the passage out of the serum and of several red blood-corpuscles and leucocytes. The connective tissue serves as a place of passage for this exudation, and preserves traces of it; yet it is hardly injured, and the essential phenomena occur in the glomeruli and renal tubes."

Unfortunately, Dr. Cornil has not concluded his paper in the December number of the *Practitioner*, and therefore I am unable to comment on his conclusions. However, the clear bubble-like globules that he mentions as being elaborated by the cells I met not only in the cells of the urinary canals, but also in the urine of different animals, and took them to be, under a power of one thousand diameters, infiltrations of fat. Probably the fact that I injected the kidney directly—a method which neither Dr. Cornil nor Browicz seems to have adopted—accounts for the slight differences that may be found in our experimental results.

Another interesting change occurring in the kidney in this disease is that of the blood-vessels. The views advanced for explaining this lesion have been the subject of much controversy. According to Johnson, we have a hypertrophy of the muscular walls of the minute arteries, which he explains by the fact that the secreting tissue of the kidney is partly destroyed, its working power is lessened, and it requires less blood. The minute renal arteries by their contractile power, under the influence of the vaso-motor nerves, now regulate the blood-supply in accordance with the diminished requirements of the gland. This regulating contraction continues and increases month after month, year after year; and the physiological result of this persistent overaction leads to a hypertrophy of their muscular walls. The rule is that the arterial hypertrophy commences when the glandular hypertrophy ceases, this being due to the fact that so long as we have compensatory enlargement of the gland-tissue, just so long does it obviate the necessity of any check action on the part of the arterioles.

According to Gull and Sutton, the changes are due to a thickening of the adventitia alone; the muscular coat, on the contrary, virtually atrophies and degenerates. These changes they conclude,

though allied with senile alterations, are probably due to distinct causes not yet ascertained, and propose for this morbid state a special term,—that of "arterio-capillary fibrosis." Lastly, Dr. A. Ewald,* who has also investigated this subject, differs from both of these gentlemen, and advances the theory that the blood becomes altered in its composition by the renal disease, excites increased resistance in the capillaries, causing as a result an increase of tension in the arterial system; and this leads to, first, a hypertrophy of the heart, and later to arterial hypertrophy. This latter he thinks is entirely to be accounted for in a mechanical way, and is dependent on the continually-increased contraction of their muscular coat, which is necessary to inhibit their extreme dilatation under the great pressure to which the vessels are exposed.

I think that all three of these causes may help produce the alteration as it occurs in the later stages of the disease, but that Dr. Johnson goes too far when he makes the assertion that the thickening only occurs in the stage of contraction and is due to muscular hypertrophy alone. Many of my sections show a thickening of the media and adventitia, and this occurring in animals eight and thirteen days after the injection. I think that Gull and Sutton are right in claiming a thickening for the adventitia, but err in not admitting a similar change in the muscular coat. My own explanation as to why we have this thickening in the early stages is, that the irritating substance in the blood accumulates in the kidney during its excretion and exerts its irritating influence upon the vessels directly; in consequence of this a molecular change occurs in their walls, by which the free filtration of serum is permitted, but the changed epithelium retards the passage of this serum into the tubules, so that all the tissues are supersaturated by a pabulum loaded with an irritating substance which acts upon the walls of the vessels as well as the other structures of the organ, and thus sets up an inflammatory hypernutrition, and in this way serves to form the lesion which the increased cardiac power would also help produce.

One other paper upon this disease I think it proper for me to mention here, and that is the one published by Drs. Da Costa and Longstreet (*American Journal of the Medical Sciences*, July, 1880), in which they express the belief that in Bright's disease, especially in the contracted kidney, there always exists a constant lesion of the renal plexus; again, that these alterations are discernible in the renal ganglia before being so in any of the other tissues; and, finally, that this diseased condition of the ganglia furnishes a clue to the alterations of the vessels in the kidney: in fact, it is the direct cause, in their minds, of all the degenerative changes met with in Bright's disease.

That they met with changes in the renal plexus I will not for a moment doubt, for I can readily conceive that the same irritation which is exerted upon the renal epithelium would also act upon the connective tissue surrounding the renal plexus, and upon such delicate and highly-organized protoplasm as constitutes the nerve-cells. But that this change precedes all others, and is the cause of the degenerative processes, I do doubt, and, as grounds for this rejection of their purely hypothetical view, will cite one of my experiments in which a cloudy swelling and fatty condition of the renal epithelium was produced within twenty-four hours after the injection.

CONCLUSIONS.

I would formulate the following conclusions:

First. That a nephritis may result whenever we have the blood going to the kidneys surcharged with any irritating material that is excreted by them.

Second. That a fatty degeneration and infiltration of the renal epithelium may be present in the very early stages of nephritis,—in fact, within twenty-four hours after its production.

Third. That the first alterations in the kidney are to be seen in the epithelium lining of the convoluted uriniferous tubules, and that this alteration manifests itself in a clouding of their protoplasm.

Fourth. That the intensity of the lesions is directly in proportion to the concentration of the poison.

Fifth. That we may have a parenchymatous nephritis without an involvement perceptible by the microscope of the interstitial tissue; but that this occurs very rarely, and only in those cases in which the poison acts either very severely and produces the early death of the animal, or its very great

* Virchow's Archiv, December, 1877.

dilution by the blood and rapid excretion render its influence transient and the animal makes an early recovery. In many cases there is an involvement of the interstitial tissue as well as of the epithelium, and cases are seen with all grades of these transitions.

Sixth. Interstitial nephritis may occur as an acute disease, but in all cases of interstitial nephritis there is always detectable by the microscope more or less cloudiness of the epithelium, and while this latter may return to its normal condition after the excretion of the irritant, the connective tissue, on the contrary, when once formed cannot again undergo reabsorption. However, the irritation exerts its influence at one time most upon the interstitial tissue, and then the red granular kidney results, or at another time it acts most on the epithelium, when we have as a result the large white kidney.

Seventh. The blood-vessel walls are thickened, and that this alteration is due to a hypertrophy of all three coats, for the irritating matter affects not one coat alone, but all three alike.

Eighth. That desquamation is due to an impermeability of the epithelium in consequence of its granular alteration, and thus the cells are forced away from their basement membrane by a *vis a tergo*.

Ninth. That parenchymatous nephritis is a very active process, as is shown by the great cloudiness of the cells and the presence of highly granular and bloody cylinders in the urine, and that the interstitial lesion is more of a passive process, resembling very much the results which we see following a passive congestion in any other part of the body.

INFLUENCE OF MATERNAL IMPRESSIONS ON THE FŒTUS.

BY D. WEBSTER PRENTISS, M.D.

IN a recent number of the *Philadelphia Medical Times* (February 11, 1882) there is a brief report of a case in which a nævus appeared on an infant, apparently as a result of an impression made upon the mother during her pregnancy.

A similar case came under my observation some years since, so remarkable that I think it should be recorded.

In May, 1872, Mrs. P., then in the fourth month of pregnancy, received a visit from a friend, Mrs. H. Mrs. H. has a birth-mark of a bright scarlet color covering one-half of the nose and extending on to the cheek. It had always been a source of great mortification; and she detailed her trials and tribulations over it and her efforts to accomplish its removal, even going so far at one time as to consent to having the nose "skinned" by a quack. This was, however, prevented by the intervention of friends.

The vivid account of her troubles, and her tears over them, naturally made a profound impression on Mrs. P.; and when her friend discovered that she was *enceinte*, she was quite horrified, and said, "Now I am afraid your baby will be marked just as I am."

Mrs. B. had the same fear, but turned it off lightly by saying, "Well, if my baby is to be marked, I will mark it here," slapping herself on the right buttock.

This interview was related to me at the time just as I have stated it, and I therefore vouch for its correctness from my personal knowledge.

In November of the same year the child was born at full term, under my care, and presented a bright-red birth-mark on the right buttock, irregular in shape, and measuring about two and one-half by two inches.

The child still lives, with its mark as bright as ever.

I have always been sceptical as to the influence of maternal impressions; but I must say that this case has shaken my opinion.

The connection between cause and effect seems so unmistakable as hardly to be doubted. The same lady (Mrs. P.) has four other children, none showing any marks whatever.

We cannot explain such cases upon any rational basis, yet they occur so frequently as hardly to admit of a doubt as to their authenticity; and the belief in the reality of the maternal impressions as a cause is almost universal among women. Such a connection, however, between cause and effect as is presented in this case is not very frequent.

In the vast majority of instances where mothers see or hear things which they are "sure" will mark the infant, they are agreeably disappointed. Many such instances have come under my observation. Where a mother has carried through her pregnancy with the dread and positive

belief that the child was marked, and still had it on her mind up to the last moment of labor, yet such fears were not realized. So that while the evidence is strong that maternal impressions may mark the offspring, still the opposite is the rule,—that in cases of exposure to such impressions birth-marks or deformities are not found.

WASHINGTON, D.C., February 20, 1882.

NOTES OF HOSPITAL PRACTICE.

PENNSYLVANIA HOSPITAL.

SERVICE OF DR. J. H. HUTCHINSON.

Reported by Dr. CHAS. H. WILLITS.

CASE OF TUBERCULAR MENINGITIS SUDDENLY DEVELOPED IN AN ADULT—CASE OF TWO RELAPSES FOLLOWING TYPHOID FEVER.

GENTLEMEN,—I bring before you this morning a case of unusual interest. It presents the following rather incomplete history.

J. R., æt. 23, born in Philadelphia, single, and a laborer, was admitted into the hospital January 22, 1882. Family history uncertain; but he always enjoyed good health until one year ago, when, after exposure to wet and cold, he was ill for two weeks with a fever and bad cough. The cough has continued ever since. The patient worked up to four weeks ago, when his weakness and cough forced him to stop. He denies all venereal history. Upon his admission to the wards he was much emaciated, face flushed, appetite poor, bowels regular, temperature 100½°, pulse 80, and respiration 24.

He had a cough, with muco-purulent expectoration; impaired resonance over both apices, more marked over the right; harsh respiration, and with prolonged expiration. Some fine râles were heard over the right apex, and fine crackling râles over the left. The urine was normal.

He was ordered sol. morphiæ, f℥ss, and syrupi prun. virg., f℥ss, t. d., and oleum morrhuæ, f℥ss t. d.

On January 25, three days later, he appeared slightly delirious and much weaker. As the cod-liver oil seemed to sicken him, it was stopped, and quinine with some stimulants was ordered.

On January 29, when, in consequence of a transfer of the wards, he came under my care, the patient appeared partly unconscious, and was hard to rouse. Face was still flushed, tongue coated with a brownish fur, dry and fissured, and sordes appeared on teeth and gums. The right pupil was more widely dilated than the left; the neck was stiff and could not be flexed; the feet were extended, and there was some stiffness of the arms, more marked on the left side, extension of which caused pain. There was dulness upon percussion under both clavicles, with crackling râles heard over the apices of both lungs.

On 31st instant he could not be roused enough to answer questions. The stiffness of the arms, legs, and neck was more marked. The same physical signs were present. The bowels were constipated, the urine passed involuntarily, and the pulse was very frequent, but not intermittent. The temperature-sheet, which I here show you, indicates a slight fever on entrance, with a marked rise of two degrees on the 25th, the first day of delirium, a fall again to normal, and a gradual rise up to the present time.

At present, as you see him to-day, he is perfectly unconscious. No efforts of mine to rouse him are of any avail.

There is no doubt, from the history and physical signs, that this man had phthisis when admitted into the wards. You will first notice his extreme emaciation and the retraction of his abdomen,—a reverse condition to that presented in typhoid fever. I will show you, also, a striking symptom well shown here. It is the redness of the skin following any slight irritation. If I draw my nail across his abdomen, you will notice very soon a red line or mark following it. From its special distinctness in meningitis this symptom is called "tache méningitique." His feet, as you see, are extended, and the tendons of Achilles are so tense that I cannot flex them. The most marked symptom remains to be told. If I ask the nurse to raise the patient, it occasions apparently great pain, and the muscles of the shoulders are so rigid that he can raise the body by grasping the head. This condition of opisthotonus, the irregularity of the pupils, and the slight external strabismus or squint of the right eye, which are shown here, are very important symptoms.

We have, then, here a case that on admission was recognized as phthisis. Three days later, delirium and the accompanying symptoms, indicating an extension of the disease to the brain, set in; and when I first saw him he was unconscious, with the symptoms I have shown you to-day even more marked. This leaves but little doubt that he has a meningitis superadded to his

phthisis, or a tubercular meningitis. Had these later brain-symptoms been present in a previously healthy man, I should have considered the case one of simple meningitis; but the accompanying phthisis points to its tuberculous character.

This form of meningitis is very rare in an adult, and, if my memory does not fail me, it is the third case I have seen in this hospital during my extended term of service. Although rare in the adult, it is not an uncommon disease of childhood, and is usually connected with important symptoms, which I will briefly notice here.

This disease comes on, not suddenly, as in our patient, but very insidiously, its onset often at times extending over many months. Vomiting, not dependent upon food in the stomach, is an important symptom. Children will scream out wildly at night as if in great danger, with a peculiar cry known as the hydrocephalic cry. The pulse in the beginning is almost always increased in frequency,—often as high as 140,—but it may again grow very slow, then again quicker and more frequent, and finally irregular and intermittent. The friends and physicians will often be misled by the appearance at times of favorable symptoms, such as a return to consciousness and less rigidity; but this improvement is only delusive, not real, and the disease will surely return again in full force.

From the slight amount of delirium in our patient, I should say that most of the disease was situated at the base of the brain. The presence of tubercles at that point does not do as much harm as the inflammation which they excite.

The prognosis of this case is hopeless; it is necessarily fatal. The treatment is equally hopeless, as there are no remedies that will remove the tubercles. I, however, ordered a blister applied at the back of the neck, and gave him f℥j of infus. digitalis four times daily, to assist in the removal of the effusion probably present at the base of the brain. I will continue this treatment, with liquid diet, although I consider the case quite hopeless. Yet we must always persevere, to guard against any possible mistake in the diagnosis.

Case II.—This young woman was in this room several weeks ago, suffering from typhoid fever, fully entered upon convalescence. At first there was some difficulty in forming a diagnosis, as she had been ill for some time before her admission, and her attending physician outside had told her she was suffering from diphtheria. In a few days after her admission, however, the disease became so well developed as to convince me it was either typhoid fever following diphtheria, or more likely a relapse of typhoid fever. The patient had a large number of passages, and passed considerable blood from her bowels. This hemorrhage was preceded by a marked fall in the temperature, and followed by an anæmic condition. Whenever a sudden fall in the temperature occurs in typhoid fever, it should always lead you to suspect the occurrence of a hemorrhage into the bowels. On examining the temperature-sheet you will notice a very decided fall just before the hemorrhage occurred, followed by a corresponding rise; a little later on, another fall and rise, indicating another hemorrhage. The temperature then gradually decreased until the normal was reached. The patient was doing very well, and was considered quite convalescent, when, some bad news being communicated to her, she had a decided second relapse. The temperature ran through the same course as before, without the fluctuations occasioned by the hemorrhages; and we had present the same symptoms of tympanites, diarrhœa, spots, etc., as in the previous attack. In my mind I am convinced that we have had here two relapses, or a relapse following upon a relapse. A relapse is not uncommon in typhoid fever, although rare in other continued fevers, as measles or variola. In these latter we may have a recrudescence of the fever, of a hectic type, but never a regular return. What occasions these relapses I cannot say. They are said to be more common after cases in which constipation has been present, as the poison cannot so readily escape from the system. They are also more apt to follow mild attacks, perhaps because in these, patients are more easily led into imprudences than in more severe ones.

There are certain points of interest in regard to lines upon the nails, which occur in typhoid fever, to which attention lately has been directed. If you will notice here, you will see a decided white line running across each nail above the lunula marking the last relapse. This only shows that, there being a malnutrition during the fever, the growth of the nails is interfered with.

The treatment in this case has been very

simple. As long as the passages contained blood, a small quantity of opium and acetate of lead was administered. This, with mineral acids, quinine, and liquid diet, was all that was deemed necessary.

TRANSLATIONS.

FRACTURE OF THE INTERNAL EXTREMITY OF THE CLAVICLE.—The following case of fracture of the internal extremity of the clavicle, in the service of Prof. Guyon, Necker Hospital, is reported in *La France Médicale*.

M., male, æt. 39, in good general health, complained of inability to use right upper extremity. He had been struck on the external part of the right shoulder by a window-sash falling from a considerable height, two days previous to admission. The pain was then very intense over the whole region of the shoulder and inferior part of the breast, and was increased by the least movement of the arm. On presentation, the position was remarkable: the arm, lightly carried behind him, was drawn very near the trunk, the patient avoiding carefully the least movement. The head, a little inclined towards the fractured clavicle, was also inclined forward. The least alteration in these positions awakened great pain. Upon inspection, the region of the right clavicle presented a limited swelling, which occupied the whole of the internal half; a large ecchymosis existed about the same points, little marked about the region of the neck, but very extensive and diffused upon the superior part of the thorax. Upon the external part of the shoulder, on a level with the summit of the acromion, there was a slight abrasion, which was the point upon which the blow was received. Upon exploration by palpation, a fine *crépitation amidonnée* could be produced under slight pressure over nearly the whole ecchymosed region. More deeply we discovered an osseous tumor lying along the line of the clavicle, and which was continuous outward, diminishing progressively. Inwardly, on the contrary, was a sudden depression, a projection which did not correspond to the intra-clavicular space, but situated upon a point which could be traced a centimetre outward. The mensuration of the two clavicles gave the same length. Movement of the upper extremity produced a sharp pain on a level of the internal clavicular region. The abduction and elevation of the arm were much less painful than the other movements, and the size of the tumor was unaffected. When, however, the arm was carried forward with adduction, the tumor was considerably reduced, but the pain at the same time was very severe.

The diagnosis was not very clear: we had some of the symptoms of the fracture of the internal portion of the clavicle described by M. Delens. We had the fusiform swelling, the absence of localized crepitation, and the position; but there was no shortening, the reduction was easy, if not complete, the tumor was attached, it is true, to the external part, but inward it ceased suddenly, and we could recognize by palpation an abrupt projection without irregularities which might readily be mistaken for the articular extremity of the clavicle. The diagnosis of luxation forward and upward was made with some reservation, but the progress of the case during the following days did not bear out this diagnosis. The ecchymosis was limited, and the whole crepitation disappeared. The osseous projection augmented quite rapidly, especially at the expense of the inferior aspect of the bone; its vertical diameter was, at the end of four days, three finger-breadths, and the fusiform swelling persisted. The projection became less and less defined and lost itself in the general swelling. The movements of the arm forward and inward no longer produced reduction, but affected the size of the tumor only a little. Eight days later the patient desired to leave the hospital. The pains were then much less severe, but all spontaneous movement was still impossible; the clavicular tumor made a considerable projection under the skin, and had then the dimensions of a mandarin orange. Six weeks later the patient returned to the hospital. The movements were yet very difficult and limited. The swelling in the region of the shoulder had almost entirely disappeared, and it was quite evident that the sterno-clavicular articulation was intact. There was, however, commencing upon it, a growth of callus large and irregular surrounding the inferior part about a centimetre outward, and at that time the mensuration of the fractured bone seemed to indicate a shortening of some millimetres.

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, MARCH 11, 1882.

EDITORIAL.

THE ETHICAL EXPERIMENTS OF THE NEW YORK STATE SOCIETY.

AMONG the remarkable acts of the New York State Society at its late meeting was the passage of a resolution requesting the Legislature to require that the conferring of the title of Doctor in Medicine should be by medical colleges, only after the examination and recommendation of an independent board of examiners, which board shall be common to all the colleges. Some of our contemporaries seem to imagine that the adoption of this resolution was a triumph for the cause of good education. To us it seems a bone thrown to Cerberus, a delusion and a mockery, the work either of some one evilly disposed, or of some friend of the cause to whom nature has denied good judgment. To join in the cry of reform, to outshout the very elect for the purpose of having some action brought about which shall be so radical as to destroy all chance of its being practical, has long been in this country a favorite device of those who would save their credit with the profession and at the same time prevent any change of system that would interfere with their pecuniary receipts in the present or the prospective future.

The apparent object of the resolution under discussion may be a very good one; but it is so plainly unattainable as to suggest the fable of the dog with the meat in his mouth grasping at the shadow, if the suspicion that it was proposed for sinister purposes be groundless. The medical colleges possess by charter the right to confer degrees; and, so long as they govern themselves in accord with their charters, it is doubtful whether any legislative or judicial body has the power to deprive them of their franchises. If the resolution were carried out, it is plain that there would be a great falling off in the crowds of students who flock to New York City, largely on account of the great ease with which the ignorant can get degrees in one or two of the most popular institutions. The colleges united are powerful, and not overscrupulous as to means when an important end is to be attained: so that it is perfectly plain that even if any legislative or judicial body had the theoretical power to take away the conferring of degrees, such power would not find any exercise.

On the other hand, if the New York State Society, now that it has concluded to receive into its fold all ranks, conditions, and sects of medical practitioners, were earnestly to set to work, it could obtain at the next meeting of the Legislature a law which should make every "legal practitioner" a member of the medical society of his county, entitled to representation in the State Society; and then give to that State Society the appointing of one or more boards of examiners, who should alone possess the right of licensing to practise in the State of New York. The colleges certainly would not offer such violent opposition to this as to a law which would deprive them of the right of selling licenses to kill in distant unprotected communities.

What in the future is to be the effect of the recent action of the New York State Society concerning homœopathy no one can tell. The scientific and practical evidence does not leave room for a shadow of doubt that homœopathy is in its doctrines hopelessly false, that most of its practitioners do not believe in its truth, and do not in their daily life carry out what they affirm; but it is possible that ostracism, though well deserved, has helped to keep the body together, and that brought into a close embrace with truth this error may

disappear, leaving only as a contribution to the common heritage those grains of verity which are mixed with its slime of multitudinous lies. For good or for bad, however, the experiment is to be tried.

If, now, the New York State Society will go a step further, and organize the whole legalized profession against the common enemy of the profession and of the laity, —the medical colleges,—possibly it may prove the pioneer of good, and not of such confusion and misrule as will result from leaving its work half done. Complete the experiment, now that it is entered upon, and let us await the result.

PROFESSOR BRIDGES.

DR. ROBERT BRIDGES was born in Philadelphia, March 5, 1806, and died a bachelor at his home, in which he had resided twenty-eight years, February 20, 1882. He graduated first from the department of arts of Dickinson College, Carlisle, Pa., and then from the medical department of the University of Pennsylvania, March, 1828. His attention was directed mainly to medical chemistry and materia medica from the start, but he was also deservedly reputed to be a judicious general practitioner of medicine. At a very early period of his professional career he became the assistant of the professor of chemistry, the late Dr. Franklin Bache, in the Philadelphia College of Pharmacy, and, when that gentleman joined the Faculty of the Jefferson Medical College in 1842, succeeded him in the chair. Dr. Bridges delivered his last course of lectures in 1879. After thirty-seven years of acceptable and efficient teaching, he resigned his professorship on account of failing health. He retained the seat, which he had long held, at the board of trustees of the college, and was always present at the meetings, unless prevented by the condition of his health. To his long and persistent interest in the institution, his unremitting attention and scrupulous integrity in the discharge of his duties, and his firmly conservative ways, the present prosperous condition of the college is to a considerable degree ascribed. As a token of the estimation of his services, the society of the alumni of the college established, March 11, 1879,—the time of his retirement,—the Bridges scholarship fund, the income from which is expended in aid of one or more poor students; and, March 31, the board of trustees elected him emeritus professor of chemistry, with an annual salary during life,—an example worthy of imitation by all bodies incorporated to impart instruction, general or technical, inasmuch as the vocation of teacher rarely affords, even to the most thrifty, opportunity to lay up a sufficient income to supply the wants of old age, however moderate they may be.

Dr. Bridges was a contributor to the *American Journal of Pharmacy*, of which he was assistant editor for many years. He edited reprints of Graham's, of Fownes's, and of Attfield's chemistries; and after the death of Dr. Bache in 1864 he was employed to do the work which had been done by him in revising and editing Wood and Bache's United States Dispensatory. He was perhaps especially well qualified for this employment through his experience as a member of the Committees of Revision and Publication of the Pharmacopœia appointed in 1860 and in 1870, and also as a member of committees of the College of Physicians of Philadelphia, to revise and prepare reports on the work to be submitted at the decennial meetings of the National Convention for the Revision of the Pharmacopœia of the United States.

In July, 1842, Dr. Bridges was elected a Fellow of the College of Physicians of Philadelphia, and was librarian of the Society during several years.

He was one of the council of the American Philosophical Society, of which he was chosen a member January 19, 1844.

In January, 1835, he was elected a member of the Academy of Natural Sciences of Philadelphia, and became conspicuous for his knowledge in all departments of natural history, and especially in botany. He prepared a list of the plants, embracing many thousands of species, contained in the herbarium—the first made—which was used for years as the guide to the collection.

He served the Society first as librarian, next as corresponding secretary, then as a Vice-President during fourteen years, and finally as President for one year, to which office he declined re-election, December, 1865. He was elected a trustee of the building fund of the Academy in 1867, and served on the building committee from the beginning of construction till the Society was established in the new building in 1876.

The conduct of Dr. Bridges everywhere throughout his life was characterized by modesty, benevolence, unselfishness, unswerving integrity in every sense, an amiable, generous disposition, and unremitting industry. His acquirements were diversified, accurate, extensive. Among medical men and pharmacists he was widely known by reputation throughout the country; and his personal associates soon came to regard him with almost affectionate respect.

LEADING ARTICLES.

WORN OUT: A STUDY IN PROGNOSIS.

NOT long since, the writer of this article was called, late in the night, to see, professionally, a gentleman very well known in business circles, who had just had a chill followed by decided fever. Into the posterior part of the right lung no air was entering, but no râle could be detected. Appropriate treatment was instituted, but the next morning it was plain that a pneumonia of moderate extent was inevitable. The case progressed without any remarkable event until the third day, when the patient, at the morning visit, said, "Doctor, I am worn out; you can't do anything; only please let me drop out easily." When a hypochondriacal man or an hysterical woman makes such an assertion, the physician in attendance may well disregard it; but coming from a cool-headed, closely calculating man of business, accustomed to face great risks, and speaking seemingly without despondency or emotion, the statement was certainly startling. Still, on close study of the case, the patient's fixed belief did not seem warranted: even upon the very day of the fulfilment of the fatal prophecy the consultant physician gave a favorable prognosis. In our certainty of diagnosis we, as a profession, are less careful and less skilful in reading the minute evidences of conditions of the system than were physicians of fifty years ago, and a case like the present, with such unexpected result, will well bear close study.

At the time Mr. X. gave his fatal prognosis he had a pneumonic consolidation limited to part of one lobe of the lung, a pulse of 96, not at all weak, a temperature of about 102°, and respiration-rate of 28 to 30 per minute; was only sixty years of age, had always led a moral life, temperate in everything except work, and had a sound heart and kidneys. It seemed impossible to believe that a fatal result was at hand, and the writer of this could not help contrasting the case with one seen not long before, in which the patient was well past seventy years of age, had been a very hard drinker,—indeed, a confirmed toper,—was so wildly delirious that it was necessary to tie him in the bed, had for days a respiration-rate of 40 to 50, a pulse of 130 to 140, and a pneumonia seemingly as extensive as that of Mr. X., and yet finally recovered under the use of a pint or so of whisky per day, aided by almost unlimited amounts of tincture of digitalis, musk, etc. Nevertheless, in spite of all that could be done, Mr. X. died, seventy-two hours after he had given his opinion of his own condition, and on the sixth day of the disease.

What was it that made the difference in the two cases?

In Mr. X. it had been early noted that both radials were markedly atheromatous; it was, of course, recognized that this enhanced the gravity of the attack; but a much more serious cause for alarm was the

inability, from the very onset of the attack, to take or digest food in any save the smallest quantities. The older man was able, without gastric distress, to drink unlimited amounts of milk-punch, whilst a few ounces taken in the twenty-four hours as the result of great urgency on the part of the nurse produced nausea and distressing flatulence with Mr. X. This difference in digestive powers may at first sight seem to account for the varying result; but why in the crisis of disease was one man able and the other unable to eat? It may well be that the real cause of the fatal result was not the inability to digest food, but was that something, whatever its nature might be, which produced such inability to digest, the man dying not so much because he was unable to take sustenance, but being unable to take food because he was dying.

It is just here that the instructive lesson is to be drawn from Mr. X.'s case, and his own words, "worn out."

Inquiry since his death has shown that a number of his progenitors have died before reaching sixty years of age. Every one acknowledges that the human race has its limit of age, and that if no accident occur the individual man must die finally because the vital forces fail,—*i.e.*, the tissues arrive at such an age that they are no longer able to appropriate pabulum and renew themselves. Every one does not, however, recognize that families, as well as the race, have their allotted stock and time of vitality, and that it is perfectly possible for one human organism to be as old at thirty as another is at eighty. Threescore years and ten is only an average of old age taken from many individualities: the one family age may be fourscore, the other threescore. In these cases the several members of a family do not die of one disorder, but of various diseases, the common factor being not the disease, but the lack of the power to resist any disease which some accident may precipitate.

The atheromatous arteries of Mr. X., in this view of the matter, had a double significance in the absence of syphilis, alcoholism, or gout: they were an almost infallible sign that the organism before us was old, and ought to have formed a more important factor in our prognosis.

Seldom does such a death as that under our notice come without warning, but usually, as in this case, under the stress of business pride, the warning had passed unheeded. Every year for two or three years Mr. X. had had a sharp attack of disease, and on several occasions had shown that same lack of reactive power whose intensity in his last illness led to the fatal result. Throughout his whole life Mr. X. had habitually allowed himself but two weeks a year of vacation, had felt himself continually strained by his work, and had recently passed through a month of excessive toil. If this organism, with the seal of old age early upon it, had been treated as old, life might have gone on quietly for years; but, when the labor of youth was voluntarily taken up because it seemed as though at sixty working-powers should not have failed, no wonder the result was as it was.

CHINOLIN AS A SUBSTITUTE FOR QUININE.

IF all that is asserted of chinolin be true, it certainly has advantages over many of its competitors for public recognition as a cheap substitute for quinine, in the abundance of its source and its antiseptic and antiperiodic properties. It was first prepared from Dippel's animal oil, nearly forty years ago, by Runge, and it is now recognized as a component part of coal-tar. Later it was obtained by distillation of quinine, cinchonine, and other alkaloids, and it may also be prepared synthetically by repeated distillations of a mixture of seventy-six parts of aniline, forty-eight of nitro-benzole, two hundred and forty of glycerin, and two hundred of sulphuric acid. It is an oily liquid, of peculiar odor; but when combined with tartaric acid as tartrate of chinolin, it forms silky crystals, closely resembling sulphate of quinine, and is soluble in water.

Chemically, as well as in its physiological action, chinolin is closely related to the cinchona alkaloids. Experiments conducted by Dr. Donath* and others go to prove that it has a noticeable lowering effect upon the temperature of the body, and that even in weak solutions it retards decomposition, whilst in stronger solutions it entirely prevents it. It also prevents the coagulation of egg-albumen and the formation of yeast-cells, and is an active poison to bacteria or fungous growths.

* Translation, Monthly Review of Medicine and Pharmacy.

The effect of chinolin on mucous membranes is somewhat irritant; but it may be taken in doses of thirty grains or more without harm. The mode of elimination of chinolin is still under investigation, it having been proved that it does not pass off in the urine, at least unchanged.

In regard to the therapeutic value of chinolin there remains much to be demonstrated, as hitherto its use has been limited to a few experimental cases. Dr. Harrington, of this city, reports the results of a trial of chinolin in the wards of the Jewish Hospital.* He employed the tartrate in four cases of intermittent, in doses of from ten to twenty grains dissolved in water. He states that the administration was not followed by emesis, except in one case, in which there was gastric irritability previously, the same patient subsequently retaining it well. It did not in any case produce ringing or buzzing in the ears; but the exhibition of the drug was followed by a lowering of the temperature and a partial cessation of the other attending symptoms. Judging from the limited number of cases, it would appear that chinolin tartrate has some antiperiodic action, but does not as yet fulfil all that is claimed for it. W. N. W.

CORRESPONDENCE.

LONDON LETTER.

ONCE more the noble old institution of Guy's Hospital has been the scene of a coroner's inquest on one of the patients who died within its walls by misadventure. A patient with typhoid fever was being treated with sulphate of quinine in ten-grain doses, each dose being kept as a powder wrapped up in white paper. So soluble a matter as quinine could easily have been in a bottle, as it usually is. This is a little matter; but if the ordinary plan had been adopted the untoward accident could have been avoided altogether. The quinine was given in powder, that is the fact. The nurse had been absent a day or two, and on resuming her duties gave the patient a powder, after which alarming symptoms quickly presented themselves, and, despite the efforts made, the patient soon died. What the nurse really gave the patient was a powder consisting of *ten grains of morphia*. What were ten grains of morphia doing in a hospital ward folded up in white paper? every reader will inquire with surprise. A great many here have asked the same question.

It will be remembered by the readers of these letters that not long ago a patient was practically scalded to death in this hospital by a nurse, and that the grand old charity has of late years been the scene of protracted civil war betwixt the new management and the existing medical staff. Guy's, it is known, is under the control of petticoats, including a matron and Sisters inspired by religious enthusiasm and entertaining a lofty contempt for the operations of the male mind, and especially the medical male mind. The ladies are in possession, and, whatever else they may be attaining, they are certainly alienating the trust and confidence of the public in their fitness to manage hospitals. Whatever may have been the defects of the management in the past, when the medical staff was habitually consulted, it did not reveal disaster after disaster of perfectly avoidable character, as is the fact now. An educated woman is preferable to an illiterate old body, not above the suspicion of a weakness for spirits, too often. That, as a broad rule, is valid enough. My own beds at my hospital are in charge of Sisters with nurses under them. My relations with these Sisters are of the very best and most cordial character. Their behavior is admirable, and I entertain a thorough unaffected respect for them. This explanation is given to illustrate the fact of no bias existing in my mind against Sisters in hospitals in the abstract. But at Guy's the Sisters have practically become masters; and what is the result? "By their fruits ye shall know them!" Misadventure, strife, uncharitableness, dissension, homicide,—these are the fruits of the new system. To go back to the mistake of giving one patient by the mouth ten grains of morphia intended for another cavity in another patient. Why were not the two powders placed in variously-colored wrappers? The dispenser explained that there was not any paper other than white on the premises. A very curious fact, certainly; though who is responsible for this, did not appear. Ten grains of morphia in a powder was certainly an unusual quantity? Yes, he said, it was. He had made inquiries accordingly, and been told that there was no mistake about the matter: it was ordered in full knowledge of its amount. He was under orders: so he certainly was not to blame. But the sooner wrapping-paper of various colors is purchased, the better.

The nurse had received no caution as to a morphia powder lying unmarked and indistinguishable among the quinine powders. Her mistake was a pardonable one. Who, then, was responsible? It is impossible to exculpate the Sister in charge of the ward. It is usual in hospitals to keep each patient's medicines on his or her own shelf at the bedhead. Had that simple rule been observed,

* Medical Bulletin, Monthly Review of Medicine and Pharmacy.

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the mistake could not have happened. Then, again, it is customary to have proper poison-cupboards in hospital wards; and surely ten grains of morphia may fairly be regarded as "poison." Had the morphia powder been put in the poison-cupboard, the unfortunate accident could not have occurred. Here two simple rules of ward-management are set at open defiance; and why? This is not easy to see, unless it was the spirit of innovation rejecting old-established rules, simply from a wanton spirit of change or unrule. Having flagrantly violated these two rules, the Sister might have warned the nurse that there was a powder of morphia, and not quinine, lying about somewhere. Even this was not done.

A young woman, a wife and mother, a valuable life in her little social circle, is poisoned in a hospital where she was being nursed through an attack of typhoid fever. That is the result of the new management so persistently upheld by the ruling powers at Guy's. Why were these drugs left to be manipulated by the ward Sister, instead of being sent up in solution, as is usual? That is a question asked by many. Ten grains of morphia would have kept as well in solution as in powder, and in solution could have been placed in a "poison-bottle," and then the nurse would have been put upon her guard. What was the powder of ten grains of morphia intended for? That is a matter which has evoked the curiosity of many. Its destination was the bladder of a patient in another bed. That is now no secret. But why were not some precautions of some kind or other observed in the matter? It is a miserable affair, look at it from what point of view one will. Nor was the treatment of the patient after the mistake occurred much better. For generations the profession has been more or less familiar with the utility of belladonna as an antidote (or rather an antagonist) to opium-poisoning, but somehow or other the medical residents at Guy's manifested no acquaintance with this fact, or, if acquainted with it, they did not choose to try the measure. One of the resident officers, in the guilelessness of his young heart, wrote to a daily newspaper to vindicate himself from any suspicion, and stated that he had tried every measure; and yet no mention of the employment of belladonna is hinted at, even. So slowly does knowledge filter through the various strata of the medical world. I suppose anything like general familiarity with the fact will only be attained by the notoriety of some trial, when the defence will be set up that belladonna was not given to the patient dying from opium or its derivative morphia, and then when the lay world is alive to the matter the profession will be awakened, and rub its eyes, and wonder how it happens that it has slumbered so long in the matter. It will be another opportunity for those who do not love the medical profession to throw a stone at it, and an instance that exoteric influence is sometimes required to institute changes that ought to spring up esoterically, or might fairly be expected to do so.

For some time past we have been well shrouded in fog, of varying density. The present condition of comparative calm with fog reminds one of the state of matters two years ago, when hundreds fell victims to the murderous fog-cloud which slowly floated back and forward up and down the Thames valley. Wherever it rested for a time death followed in its wake. Capillary bronchitis was the consequence of the irritation set up by the peccant matters; and how fatal a malady this is, is well known to all who have been brought in contact therewith. So serious are the recurrent fogs we have experienced of late years that a strong movement is set on foot to deal practically with the dangerous nuisance, and a "Smoke-Abatement Exhibition" is now going on at South Kensington under royal patronage. A very interesting exhibition it is, and all kinds of stoves are there, each exhibitor vying with his neighbor to produce the most perfect combustion of coal. Especially is the anthracite, or smokeless Welsh steam coal, advocated. The difficulty of this coal is to persuade it to light: once agoing, it is a splendid article. No coaxing will induce it to take fire under the ordinary domestic regulations as to fire-lighting: a "blower" is requisite for its ready combustion. But carefully packed upon a fire lighted by ordinary house-coal, it burns without smoke, and with a minimum of ash. The English domestic is, however, a thoroughgoing conservative, tenacious of what is well established and time-honored, and not given to adopting any new-fangled notions hastily or without proper consideration. Anyhow, there is no harm in attempting to disseminate a little knowledge which may filter downward in the course of time and reach the ordinary domestic about the millennium. Lectures are given bearing on the subject, and the writer was requested to give a lecture upon "The Effects of Coal-Smoke upon the Human Body." So the subject was cogitated over, and the lecture ran upon the following lines, which may not be altogether uninteresting to the people of Pennsylvania, which possesses liberal supplies of coal within its borders. A fog, as understood in London, is a smoke-cloud from the quantity of matter furnished by the myriads of smoking chimneys in this large aggregation of human beings. Rome burnt wood, and from its position did not require so much artificial heat as London does. The effects of "sea-borne coal" in relation to fogs have long been recognized in London. Now the bulk of coal is brought by railway, and its consumption is enormous, as the Britisher dearly loves an open fire. Such fire may be waste-

ful, may increase immensely the amount of smoke in the air; but John Bull is distinctly conservative in the matter. The flickering of the flame, the ruddy glow of the open fire-grate, are dear to him. No pun is intended here! He looks upon the stoves of his Continental neighbors as convincing proof of their inferiority, and regards them with aversion. Cheap coal at home causes him to regard the stove as a mean piece of economy of which he has no occasion to be guilty. He does not like the foreign-looking thing, and will have none of it. However, the amount of fog we have recently experienced is compelling him to give some attention to the matter. When traffic in the city is arrested and business all but suspended, the commercial instincts of the old man are aroused; and he is beginning to believe that he is losing something by these fogs. If that impression could only root itself in his brain, he would begin to bestir himself in earnest. Knowing the sensitiveness about the region of his left trousers pocket, my lecture headed in that direction, so far as the subject would permit, by including the effect of coal-smoke upon the nervous system. Commencing by alluding to the use of tar-smoke in plagues, either amidst human beings or cattle, and the belief in the utility of fumes in the curing of meat, as ham and haddock, as well as the effects of the creasote so produced as a disinfectant for the living when exposed to contagion (as a preservative and a hygienic measure), the lecture went on to point out the effects of unconsumed matters as mechanical irritants to the air-passages. The black lungs of Newcastle miners are due to the deposit in them of carbon, as was shown by the late Sir Robert Christison long ago. (Dr. Peacock found that the phthisis of Cornish miners was not, however, connected with any foreign matters in the lungs,—an observation not destitute of interest.) "Black lungs" are quite common among persons who have lived any length of time in London. Then the chlorine given off and the sulphurous acid are chemical irritants not without potency. Chronic bronchitis, varying from a serious matter down to a mere inconvenience, is common among the inhabitants of the numerous manufacturing towns which lie on each side of the backbone of England. In the morning everybody seems to be hawking and spitting, till the streets are simply revolting and utterly unfit for ladies with long dresses to walk upon. When resident in Leeds, twelve years ago, the atmosphere drove me to adopt a respirator (my nose not being the family ploughshare and being disproportionate to my thorax) as a preservative measure, and experience encourages its use as such. After a fog the nostrils are like chimneys, and are lined with a layer of black smut. The expectoration is black from the amount of carbon arrested in the mucus of the air-passages. For a day or two after exposure to a smut-laden atmosphere, black phlegm is brought up. That soot is an irritant to the tissues is demonstrated by the fact of "chimney-sweep's cancer," a well-recognized disease. Yet if the curious reader will look into Pereira's large work on materia medica, he will find directions for the preparation of a "soot ointment," and even an infusion of soot as a tonic; while soot as tooth-powder was in quite common use a few years ago. So much for fashion in medicaments. Chronic bronchitis is not the only form of disease of the respiratory organs instituted by inhaled irritants: cirrhosis of the lungs is also the result thereof. "Sheffield grinders' asthma," "potters' phthisis," from the scouring of "biscuit-ware," "millstone-makers' phthisis," the phthisis of linen-dressers, of feather-workers, of millers and bakers, all are well-recognized types of disease. Beyond these direct effects upon the respiratory organs, the products of coal-smoke have other consequences upon the human frame. Coal-gas is a deadly poison, suspending consciousness first and then arresting the respiration and circulation, as is well known. Carbonic acid gas may be as fatal as the fumes of charcoal, often utilized for the purpose of suicide. In a less degree it produces lethargy and sleepiness; and sometimes the imperfect ventilation of the building, rather than the lack of interest in what is said by the speaker, is the cause of an inattentive, slumbering audience. A more decided poison still is the less oxygenized carbonic oxide which is the outcome of the imperfect combustion of coal. This and coal-gas are worse than carbonic acid in their effects upon the nervous system. Imperfectly consumed coal-products are more deleterious than the completely oxidized carbonic acid. Any one who has watched a coal fire burning must be familiar with the little volcanoes throwing out a jet of gas, which are found in partly-consumed coals,—now a dark puff, now a flame like that of a gas-burner. Here is coal-gas given off without any mistake. The effects of these olefiant gases and of carbonic oxide upon the nervous system, in acute poisoning, are well known; but the chronic effects of them in high dilution in the air are less familiar to us. "Depression of spirits and confusion of thought" are part of the subjective phenomena of such poisoning; and any one who has tried to do a square, honest day's work in a thick fog knows something in the matter. Depression, a sense of intellectual incapacity, and irritability of temper are the nervous outcomes of air so polluted, to say nothing of the local effects of a stinking fog, when the air-tubes feel as if wires were introduced into them, and the back of the pharynx is irritated by carbon particles. A pleasant condition of affairs for a huge hive of workers, as London unquestionably is, despite an

immense idle population who scarcely know how to kill their time. Worker and idler alike are rendered uncomfortable. Nay, more. Some years ago the fog existing during the famous Islington Cattle Show set up such disturbances of the respiratory organs that many of the beasts had to be slaughtered; and many a person who comes up from the country for a few days on business or pleasure is killed by fog. The stranger within the gates perishes outright; the regular inhabitant is more slowly poisoned. His nervous system is depressed and unstrung; his temper is upset; and, still worse, his appetite is impaired. London fog is certainly not appetizing, but the very reverse. Just as "nausea and vomiting" are part of carbonic oxide poisoning in its acuter forms, so anorexia and a distaste for food are the outcomes of more chronic intoxication. Alcoholic beverages have a clear *raison d'être* in fogs, and in smoky neighborhoods the consumption of alcohol is pronounced. Nor is this to be wondered at, when to impaired nutrition is added the depression produced by a nerve-poison, and the acute loss of appetite created by a London fog.

The next lecture was delivered by Prof. Judd, who explained that the fireplaces of the half-million households of London were constructed only to burn the soft, smoky household coals now in use, and not the hard, smokeless coal which ought to be burnt, in all large towns especially. He went on to say that there was every reason to believe that under London, or its immediate vicinity, there lay beds of this very identical hard coal, and that, too, at distances below the surface which would render them quite easy of access. When these coal-beds were worked, the people of London would have coal much cheaper than heretofore, and this variety of coal would give off infinitely less smoke; and so the metropolis would be relieved of the fogs which now render it almost unfit for habitation. Colliery-chimneys on the Surry hills, or along the ridge to the north of London! Horrible thought for Mr. Ruskin or the æsthetics! But for the myriads of humble toilers in London, good news; and for all, workers and idlers alike, the prospect of being relieved, if only partly, from these hateful fogs to which we are now so subject, is too good to be credible, almost. As to those who already suffer from disease of the respiratory organs, fogs are a source of the greatest danger to life. At the Victoria Park Hospital, recently, a severe fog caused four consumptive patients to collapse fatally, while many others were very ill.

J. MILNER FOTHERGILL.

THE death is reported of the famous physiologist Dr. Theodore Schwann, in the seventy-third year of his age.

PROCEEDINGS OF SOCIETIES.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

A CONVERSATIONAL meeting of the Society was held at the hall of the College of Physicians, Philadelphia, December 14, 1881, Dr. Albert H. Smith, President of the Society, in the chair. Dr. J. William White read a paper on "The Prevention of Syphilis" (see page 225), discussion of which was indefinitely postponed at the request of the author, and the subject referred to a committee, to report not earlier than one year from the date of appointment.

Dr. E. L. Keyes, of New York, by invitation read a paper on the "Treatment of Syphilis" (see page 337). A vote of thanks was tendered to the lecturers for their interesting communications.

DISCUSSION UPON THE TREATMENT OF SYPHILIS.

Dr. L. A. Duhring spoke in corroboration of the views expressed by the lecturer, and said that he was impressed, from experience, with the propriety of the measures recommended. He had struck the key-note,—moderation in the use of mercury. There can be no question that mercury is the remedy in syphilis. In thinking over the many cases that had come under his observation during the past fifteen years, he could recall few which had been over-treated by mercury; the majority did not get enough. He had rarely seen any serious or permanent ill effects from mercury, even in the wards of the Philadelphia Hospital. Urging the free but moderate use of mercury, he said that it was rare in private practice for patients to get enough mercury or to continue it for a sufficient length of time. The great trouble is that patients drop off the treatment at the end of a year or so, believing themselves cured, or, influenced by the remarks of others, they disregard the importance of continuing, and the usual sequelæ follow.

Notwithstanding this, there are some cases in which success does not attend the treatment, even when given as directed by the lecturer. In several cases patients had taken the remedies until within the second year; then, almost while taking the mercury, certainly shortly afterwards, they showed obscure nervous symptoms. Cases like this tend to shake confidence; but in discussing the question failures should be reported as well as successes. At the same time, any one who will take the treatment of Dr. Keyes would, he thought, be satisfied with it after a few years' trial. The difficulty is in keeping up the treatment for a long enough time, for the patients get discouraged and go to other physicians, who, perhaps, would not give them the same advice.

With regard to the benefit of local treatment in the syphilodermata, and more especially the squamous eruptions, he would scarcely lay so much stress on local applications. The more he saw of diseases of the skin of a syphilitic character, the more impressed he became with the value of judicious constitutional treatment, and less with that of local treatment, except, of course, where it was obviously demanded. In this connection he reported a case of a gentleman who had been under the care of a number of physicians, with a squamous syphiloderm of both palms, which had been looked upon as chronic eczema and received local treatment for fifteen months without benefit. When he came under the speaker's care, the man was perfectly sound except the skin affection. He was ordered iodide of potassium (gr. v) and biniodide of mercury (gr. $\frac{1}{16}$) three times a day, and in the course of a few months he recovered entirely, the local treatment having been totally abandoned. This occurred several years ago, and the patient, he believed, was well to-day.

The object of these remarks was—first, to corroborate the views expressed by the lecturer; secondly, to insist upon the importance of continuing the treatment for several years; and, finally, to acknowledge that it sometimes unaccountably fails.

Dr. Carl Seiler said that he was glad to hear the lecturer speak of the value of local treatment in syphilitic manifestations, for he had read a paper before the American Medical Association upon Syphilitic Laryngitis, wherein he advocated local treatment, and was severely criticised by a large number of the members for this recommendation; but he held to his own opinion, and would thank Dr. Keyes for substantiating this method of treatment.

Dr. Chas. B. Nancrede referred to the use of the hæmacytometer and the fact that in syphilis mercury increases the number of the red blood-cells. He said that he could not understand the prejudice against mercury: he found it essential in syphilis, and also used it in eye-diseases where there is no suspicion of syphilis.

Dr. Levis inquired the value of iodoform in syphilis; and the President suggested discussion of the treatment of congenital and hereditary syphilis.

Dr. John B. Roberts said that some five or six years ago he had been struck with Dr. Keyes's article on "The Treatment of Syphilis by Tonic Doses of Mercury." He was astonished at this use of the word "tonic," as it entirely differed from his former views.

He inquired of the lecturer whether he finds that scrofulous patients, often merely hereditary syphilitics, respond in any different manner to treatment than subjects of the acquired form; secondly, as to the combination of iodine and mercury in prescription; thirdly, as to the comparative values of the iodides of potassium, sodium, and ammonium; fourthly, as to the use of mercury by suppository; and, finally, as to the value of sulphate of copper in syphilis.

Dr. W. S. Little said that in treating syphilitic eye-disorders, where the disease is limited to the sclerotic and cornea, mercury is the treatment; but where the iris, ciliary body, and choroid are involved, the combination of mercury and iodine is required.

In syphilitic keratitis the combination of mercury with any form of tonic will meet with prompt improvement: he usually combines the mercury with quinine. In syphilitic retinitis, iodide of potassium with strychnia is preferable, strychnia being given in the later stage of the treatment.

Dr. J. William White said that the propositions of the lecturer admitted of very little discussion, as no one with experience in syphilis questions the advantages of methods adopted; but with regard to the *theory* of treatment he would take advantage of Dr. Keyes's presence to ask for information.

One of the most satisfactory theories in its explanation of the effects of treatment is that depending upon the application of what is known as the lymphatic theory of syphilitic infection, and by which the relief afforded by mercury is attributed to the well-known influence of mercury upon newly-formed structures wherever they are found. By this theory syphilis depends upon the introduction into the organism of a diseased cell, which, at its point of entrance, produces a chancre. From thence it passes by the lymphatic channels, producing bubo in the groin; then, entering the general circulation, it especially affects the white blood-cells, combining with or infecting the germinal material, to which it conveys its specific properties of rapid multiplication and immature development. The growth and accumulation of these cells are the source of all the secondary symptoms of syphilis. The functions of the lymphatics are interfered with during this stage, and later they are contracted or obliterated, as a result of long-continued irritation, and the imperfectly-formed or waste tissue accumulates at various points, producing the so-called tertiary symptoms, there being no discoverable difference, histologically, between ordinary waste elements and those resulting from the late developments of syphilis.

The good results obtained by the use of mercury are attributed to its eliminative effect upon the newly-formed cells, the fatty degeneration and absorption of which it strongly favors. The iodides are also moderately serviceable in the early stages, for the same reason. In the later periods the theoretical indication would then be to flog up the crippled lymphatics and stimulate them to more active absorption, so as to prevent accumulation of nutritive waste, or, in other words, to

prevent the development of gummata, nodes, and other tertiary phenomena. This offers an explanation of the usefulness of the iodides in the later stages.

It is certainly a very beautiful theory, but needs further investigation to insure its general reception by the profession and to fully establish it. The opinion of the lecturer was requested upon these views.

Dr. Keyes, in closing, thanked the gentlemen who had taken part in the discussion for the interest shown and their courteous treatment. Before answering queries proposed, he exhibited two specimens of iodide of starch, referred to in the paper, made according to McCall Anderson's formula (containing five per cent. of iodine), except that he had lately substituted boiled-rice starch for the customary raw-wheat starch in the manufacture. It agrees with the stomach, and is an elegant preparation, but its great objection is its bulk where large doses of iodine are required. It cannot take the place of iodide of potassium, but under certain circumstances is an admirable substitute. In reply to Dr. Duhring, he said that he would not pretend that the tonic treatment or any other would always keep off the late effects of syphilis, but he would claim that it is nearly always efficient in preventing grave late symptoms, that it is harmless, and better borne, as a rule, than any other treatment, and in the majority of cases most satisfactory to the patient and physician. With regard to the local treatment he was misunderstood. He wished to say that if a patient is already upon the tonic treatment and an isolated manifestation should crop out, instead of running up the mercurial dose he would merely add a local to the general treatment. For local applications he liked best the acid nitrate of mercury diluted to different strengths, sometimes used pure, at long intervals, upon isolated mucous or cutaneous lesions, but diluted one in eight or more parts of water for frequent use. Local treatment with small doses of mercury he considers better than the attempt to control symptoms by large doses, which disturb the general health. The observations with the hématimètre showed that large doses do have this effect upon the system. When the patient has a local eruption, he believed in general treatment, but not to the exclusion of local treatment. Rather than increase the doses, he would extend the time for the mild course. The bichloride of mercury has been recognized as a tonic, even in popular remedies; for instance, Swaim's Panacea, a tonic much used by the late Valentine Mott, containing one-half to one grain in three ounces of compound tincture of cinchona, He thought highly of its value for strumous children. The value of small doses of mercury is generally admitted in fibrous phthisis, in degeneration of the kidney, where it is given in combination with tincture of the chloride of iron.

The use of mercury as an antisyphilitic is well established. The reason why he had referred to the "tonic" use of mercury in syphilis was because he had been misunderstood. Some of the journals had said that he had claimed that mercury cured syphilis *because* it was a tonic. This was wrong. He claimed that it cured syphilis and also was a tonic. He had tried mercury upon himself, not having syphilis; he wished to ascertain its physiological effect. The same experiment was tried upon Dr. Stimson; and in both instances the mild mercurial course increased the corpuscular richness of the blood.

Iodoform is an exceedingly good application for syphilitic sores; but its smell forbids its use in private practice among decent people. Internally, he had given as much as five or six grains three times a day, without any material advantage, in syphilis. He had subsequently used it in small doses in chronic tertiary affections of the tongue, as recommended by Berkeley Hill, but had not conceived a favorable idea of its value. He did not see very much of congenital syphilis. He had not seen, out of fifty or sixty cases, any syphilitic children in the families of young men who, having followed out his course, were afterwards allowed to marry.

Women possess the power of procreating syphilitic offspring for a longer period than males. For infantile syphilis he had used various forms of mercurial inunction and gray powder internally. Young children tolerate mercury very well; but he preferred the use of the bichloride of mercury in solution (distilled water, ℥viij to xij; mercury bichloride, gr. i), giving a teaspoonful every hour. The youngest babies will take one-ninety-sixth of a grain every hour for a number of days without showing the slightest inconvenience of bowels or stomach. This is not to be kept up continually, but is diminished according to its effects upon the symptoms. He had even given it during diarrhœa in summer, without increasing the looseness. He liked this form of mercury for children. As to whether it is a tonic in the case of scrofulous children, he could only refer to its employment in the tonic of Valentine Mott, which is used very frequently for secondary engorgement.

With reference to the mixed treatment, he sometimes uses the protiodide of mercury and the iodide,—the former in pill, the latter in solution; but he prefers the combination of biniodide of mercury and iodide of potassium in the same mixture. He occasionally uses small doses of calomel, frequently repeated, when he wishes to obtain a special effect; sometimes he resorts to fumigation. He gives mixed treatment, as almost every one else does, and

uses inunction, with the iodide internally, and so on. He had seen it noticed in the books that when taking the protiodide a patient should not take iodide of potassium, for fear of forming a biniodide in the stomach. He had never known this to happen, although it occurs when the bichloride is added to a solution of iodide of potassium; but instead of this he usually prefers giving the biniodide directly (one-thirtieth to one-fiftieth grain), with a certain amount of the iodide of potassium, according to circumstances.

With regard to the comparative value of the potassium and sodium salt, they contain about the same proportion of iodine, but the sodium salt agrees better with the stomach, while the potassium is more efficient. He had used the sodium salt and the biniodide of mercury by enema when the stomach would not tolerate the remedy, but would now use the iodide of starch in such a case. He had not used mercury by suppositories except in very rare instances; inunction is better by the skin than by the rectum. The external use of bichloride dissolved in cologne-water is a very elegant method of application, and exercises a certain amount of specific action. Sulphate of copper he had never tried; he had formerly used the chloride of gold, but was utterly disappointed; perhaps he will try the sulphate of copper.

With regard to Dr. Risley's remarks, he said that he saw very little of eye-disorders, and perhaps gummata of the eye were different from periosteal gummata, but the latter will fade away in a few days if the iodide is pushed promptly; also under mercury they may be made to disappear, but less promptly, and sometimes, apparently, not at all.

Mercurial fumigations are very valuable in chronic diseases of the skin, and gummata of the skin, or brain and meninges; but his experience is that iodide of potassium gives a more permanent result, and he had come to the conclusion that the more gummatous the lesion the more use there was for iodide of potassium, and the less for mercury.

As to the theory of Dr. Otis, it has been well described as a beautiful theory, but thus far it rests solely upon induction. That the mercury causes fatty degeneration of the syphilitic deposit may or may not be true, but, if so, the small doses will cause fatty degeneration of those structures, and not of the healthy ones. This is not of much importance; but the other view, that the iodide acts by opening the lymph-channels, and sweeps away the accumulation of waste material, seems to be overturned by the fact that mercury will cure gummata without opening the lymph-channels, while iodide of potassium will cause early eruptions to disappear, without inducing fatty degeneration.

He did not deny it, but did not consider it fully established, or essential to a fair knowledge of the correct treatment of syphilis.

SOCIETY FOR MEDICAL IMPROVEMENT, BOSTON.

January 28, 1882.

DR. C. M. GREEN read an interesting *résumé* of thirty-five obstetric cases, of which twenty-seven were multiparæ and eight primiparæ, all of them being provided for purposes of clinical instruction by the Boston Dispensary. There were twenty-three occipito-left-anterior presentations; six occipito-right-anterior; four occipito-right-posterior; sacral right anterior, and sacral left anterior, one each. No deaths, either maternal or infantile. There were two cases of twins, one having a single large placenta, with divided ridge and two cords; the other a double placenta and two cords. There was one case of miscarriage, the fœtus being dead, resulting from over-fatigue, no motion having been felt for one month previous to abortion. One case of brow presentation. Labor had been in process thirty-six hours before the head passed superior strait. During first strong pain head was flexed by pressure upward on frontal bones; occiput then rotated forward and delivery ensued. This was supposed to be one of those cases in which, the pelvis being slightly narrowed, the head becomes extended in endeavor to pass the brim, and the bitemporal diameter passes where the somewhat larger sub-occipito-bregmatic will not. Schroeder considers this mechanism good evidence of a slight pelvic flatness. Such malpresentations frequently correct themselves in cavity of pelvis, and thus, unless detected early, escape notice. There were three cases of instrumental interference:

a. Multipara, æt. 27 (three out of five previous children still-born), fifteen hours in labor when pains became ineffectual. Head on the perineum, and child dead. The attending student called a physician, who applied forceps, and delivered the head, but was unable to deliver the body. One drachm of fluid extract ergot was administered, and a putrid fœtus delivered by traction. Placenta being adherent was extracted by hand; was small and shrunken. Patient was doubtless syphilitic. Case mentioned in order to criticise application of forceps to a putrid head; the cranioclast, it was thought, would have been more suitable and safer. Dr. Green also objected to the use of ergot to stimulate a uterus to expel even a putrid child; for if placenta be adherent, as in this case, it is more difficult of removal from a contracted uterus.

b. Primipara, æt. 24, at expiration of forty-eight hours of labor, first stage was not completed. Had received forty-five grains chloral in three doses; had also taken ether. Pains being inefficient, dilatation was completed manually, and forceps applied. To avoid rupture of perineum, it was incised laterally. Placenta retained. Crédé's method failed. The hand was therefore introduced, and found placenta adherent and encysted. Constric-

tion was dilated and placenta easily removed. Patient received carbolized douches for a week, and made good recovery.

c. Primipara, æt. 24, in labor twenty-four hours, os about half dilated. Head had not entered pelvis, and, there being a sufficiency of hydramnios, membranes were ruptured. Head now engaged in pelvic brim, but, because of uterine inertia, failed to advance. After lapse of a second twenty-four hours, patient's condition being good, forceps were applied at superior strait. Strong traction delivered a large child, but perineum tore as far as one and one-half inches up the rectum. Primary operation on perineum was deferred; urine passed by catheter for one week; vagina douched with carbolized injection during two weeks. Then found that rectal tear had completely healed. On rising, patient had one involuntary movement; has since had perfect command of sphincter. To prevent future displacements of womb, operation for restoration of perineal body will be done at completion of lactation. Examination disclosed a stellate laceration of cervix not demanding interference.

Students were directed to prevent passage of head through outlet until perineum became fully dilated. Ordering patient to cease bearing down, and counter-pressure on head, when needed, were the means adopted to this end. Were also taught to extrude head by gentle manipulations between pains and after occiput had passed under pubic arch. Results: in four of eight primiparæ, perineum not ruptured; in two cases, slightly torn, no operation needed; in one case, ruptured nearly to anus, operation refused; in one completely ruptured. In twenty-six multiparæ, perineum not ruptured, possibly because sufficiently torn in former confinements.

There was one case ot post-partum hemorrhage. Patient cold, restless; bathed in cold sweat; pulse 100. Ergot, stimulants, ice in the vagina, all failed. Hemorrhage finally arrested by application of persulphate of iron on a swab.

No case of septicæmia, owing to faithfulness of students in charge, who, in many cases where lochia became fetid, administered carbolized vaginal douches. Thirty-two patients suckled their infants.

One case showed how much puerperal women may bear without evident harm:—Multipara, æt. 33; been twice in insane asylums within four years. In July last, incarcerated for extreme violence while under influence of alcohol; discharged next day. At all times irritable and jealous; beat her children without mercy. Confined in August, six weeks after discharge from asylum. On night after labor demanded liquor, then rose and washed soiled linen by placing it on the floor and pouring water over it; also cooked and ate beefsteak. Child was fed on watermelon and whisky, and finally quieted with laudanum. Next day woman looked wild, but showed no ill effects; pulse and temperature normal. Considering her history, this outbreak was thought transitory mania, alcohol being the exciting cause.

In reference to Dr. Green's paper, Dr. Francis Minot said that increased use of forceps of late years not only saved much suffering, but rendered convalescence freer of danger and more rapid. He probably used forceps more frequently than most practitioners. In two hundred and eighty-two private cases he delivered with forceps ninety-two times,—thirty-two per cent. He thought his results as favorable as those of most physicians. No death, nor unusual number of lacerations, floodings, etc., had occurred at his hands from the use of instruments. In a series of one hundred and fifty-one labors there were twelve serious hemorrhages; in three of these cases forceps were used, but in each instance for exhaustion, inefficient pains, or tedious, difficult labor. In the ninety-two forceps cases, fifty-eight per cent. were male children, forty-two per cent. female. In nineteen cases occiput was posterior,—eleven male, eight female children.

Had often incised the thin edge of perineum during labor, in primiparæ, but, judging from his record, with little benefit. In many cases it did not prevent a tear in median line, occasionally to a considerable extent.

In treatment of post-partum hemorrhage he referred to the fact that it was rarely fatal, and that the large number of remedies thought successful showed that the condition probably was but little influenced by any of them. Was accustomed to rely upon compression of aorta, which is easily done, and he thought it quite efficient.

Alarming symptoms succeeding upon use of carbolized vaginal injections in two cases, during convalescence after labor, proved to him that the procedure was unsafe. Where there was fetor in discharges he preferred a swab wet with carbolized solution. In the majority of the forceps cases the head, naturally, was on the perineum, and the instrument was used to prevent a tedious and, as he thought, a needless delay.

Dr. W. L. Richardson said he had often remarked a tendency to post-partum hemorrhage after the use of ether. Very rarely used ether; had never seen a hemorrhage after labor. During first stage nearly always gave chloral in gramme doses every twenty minutes until three doses had been taken, and thus avoided use of ether. Uterus being a hollow muscle, could not understand why ether should not relax that organ as well as any other muscle of the body. Agreed with Dr. Minot in regard to frequent use of forceps. They not only prevented suffering, but shortened convalescence, and enabled one to protect perineum from injury. Was especially interested in case of Dr. Green's in

which occiput presented posteriorly. Thought physicians erred greatly in being contented with presentation without ascertaining the position as early as possible. In cases of occiput posterior we should remember liability of extension of head. Application of pressure on anterior portion, as in Dr. Green's case, promotes flexion, and thus permits subsequent rotation of occiput forward. In cases seen in consultation, when this pressure had failed, he had been able to flex head by forceps applied with curve reversed. In examining statistics had been struck by large number of cases of septicæmia following birth of putrid child. Had, therefore, for past few years, at Boston Lying-In Hospital, always used intra-carbolized injections after birth of putrid fœtus. Had thus avoided high temperatures which followed such labors before the use of injections.

Dr. Reynolds refused to admit danger of hemorrhage from ether, provided the drug were administered with ordinary discretion. He would weigh strain upon mother's system, induced by unrelieved suffering, against any supposed relaxation of uterine fibre, caused by proper use of the anæsthetic, as a factor of equal if not greater importance in causation of hemorrhage. While rarely conducting labor of any length without anæsthesia, had seldom seen serious hemorrhage. Believed that, in private cases, careful management, increase and saving of strength of patient before labor, watchfulness of the process, and proper tarry with patient after delivery would lessen liability of bleeding. Would use moderate dose of ergot after free use of ether if uterus were first entirely emptied.

Endorsing Dr. Richardson's remarks on the necessity of early detection of the presentation, he emphasized the distinction between mere posterior variety of either left or right cranial position, and the same variety when combined with loss of due flexion. In the former there is no interference with normal mechanism, unless it be the possible loss of the few moments occupied by passage of the occipital end through three-eighths of pelvic circle instead of the shorter space. In the other case, flexion being much lessened, and posterior fontanel being discovered only with much trouble, or not at all, everything indicates a difficult expulsion, and the perverse rotation which will bring frontal end of cranium to front wall of pelvis, and a probable emergence of occiput backward, over the perineum.

The greatly increased suffering, and risk of injury to patient, thus created, should be remembered by attendants. In this, as in a thousand other obstetrical embarrassments, thorough care on part of accoucheur involves presence early in labor, and watchful and alert mind.

In the few obstetric cases reported by reader, two instances of adherent placenta were hardly to be expected. In one of them syphilis existed, and was sufficient cause. In the other, real adherence probably did not occur. True adhesion of placenta bears small proportion to the large number found in obstetric reports, the majority of these placentæ probably being held by mere suction, as a boot clings to deep clay.

Dr. Charles H. Williams, having proved its use in affections of the conjunctiva, suggested a saturated solution of boracic acid as substitute for carbolized injections after labor.

Dr. J. Collins Warren, in reference to dangers supposed to have been caused by carbolized injections, said that at present surgeons inclined to use weaker solutions and avoid carbolic acid in washing out internal cavities, as the bladder; had used boracic acid as injection for bladder.

Dr. Doe illustrated danger of vaginal injections by case of multipara, whose labor had been normal. Vaginal injections, carbolized 1 to 120, were ordered thrice daily. On morning of sixth day during injection, patient said water had gone up to her stomach; complained of abdominal pain, which became intense and passed into semi-collapse. Three hours later was very pale, pulse thready and weak, abdomen distended, tympanitic, very sensitive to percussion. Pain more moderate. Stimulants, morphia, and hot fomentations being used, the pain, inflation, and soreness disappeared within twenty-four hours. No ill ensued.

Said also that in his experience vomiting had been more intractable and persistent in twin than in single pregnancies, and asked the experience of members present.

Dr. Minot's experience had been the contrary of that of Dr. Doe.

Dr. Green thought the nausea and vomiting common to early months of pregnancy were probably due to sinking of uterus in pelvic cavity, and that hence it was logical to expect greater intensity of these symptoms in twin pregnancy, uterus naturally being heavier.

NEW YORK COUNTY MEDICAL SOCIETY.

REGULAR MEETING, FEBRUARY 27, 1882.

DR. F. R. STURGIS, PRESIDENT, in the chair.

THE paper of the evening, entitled "The Permanent Removal of Superfluous Hair," was read by its author, GEORGE H. FOX, M.D.

The growth of hair upon the female face, of which the paper principally treated, was of frequent occurrence. There were hundreds of women who might, if they would, have a long beard; and the number of those who had some abnormal growth of hair upon the face was beyond computation. This abnormal growth of hair was not always a trifling matter, although usually treated as such by

the physician when consulted for its removal. It was often a great annoyance, affecting the patient's disposition and prospects in life, thus indirectly ruining her health, perhaps inducing melancholia. His method of operating for the removal of superfluous hairs was by electrolysis. The method was simple and could be employed by any physician, although dexterity could be acquired only by experience. To the negative cord of an ordinary galvanic battery a fine needle was attached for introduction into the hair-follicle. An ordinary fine cambric needle, which had been recommended, was too stiff, and answered the purpose not so well as the fine hair-like flexible steel guide which he used. The coarser and stiffer needle was more liable to produce inflammatory reaction and permanent traces after the operation. A high reclining chair, a good light, and a steady hand on the part of the operator were important. At present it was his advice not to remove the hair until after it was loosened by electrolysis. The number of hairs removed at a sitting should be only about thirty or fifty, although he had removed more than two hundred. If the operation were very skilfully performed, the little punctate marks which had sometimes been left, and could be seen on close observation, would be avoided. At first the patient usually was a little nervous and sensitive, but soon became accustomed to it, and did not complain of pain. The author read the history of a number of cases in which he removed from fifty to over five thousand hairs from the face of different women,—in some with the result of permanent removal, in others nearly so, the treatment not yet having been completed. When a second crop of fine soft hair came out at the seat of the original, it was removed in a similar manner, with permanent results.

It was a question as to how electricity destroyed the growth of the hair,—whether by thermic or by electro-chemical action. Dr. Heitzmann objected to the term electrolysis, claiming that the heat generated in the needle had the effect to destroy the tendency to growth of the hair. Dr. Fox did not think the amount of heat generated in the needle was sufficient to have this effect, and that the employment of the term electrolysis, because of the meaning it conveyed, was proper.

As to causes of excessive growth of the hair he was unable to determine. Some in whom it occurred were debilitated, others not; some nervous, others not; some dark-complexioned, others light; some young, others older; some married and mothers, others single. The idea that a beard in the female was associated with a masculine character was not founded on fact, for some of his patients were most feminine in disposition. That it was dependent upon the condition of the reproductive organs was, in his opinion, doubtful. The relation had not been proven, only in exceptional instances. Its relation to derangement of the nervous system had been studied, but not sufficiently. Excessive growth of the hair, whether in the male or female, was an aberration of nutrition, and not a sign of excessive vitality. The Samsons of the present day were usually clean-limbed and of moderate hair-development.

DISCUSSION.

Dr. HEITZMANN referred to the introduction of depilation by electricity in this country, and agreed with Dr. Fox that to American dermatologists honor was due for material progress in this direction. Since Dr. Fox had shown him the details of the method, he had employed it a great deal, and with the most gratifying results. He had seen scars produced in but one instance, and then the disfigurement was observable only on close inspection. As Dr. Fox had stated, he believed in the thermic effects of the needle. As to what part any other influence might play, he could not tell.

Dr. GEORGE M. BEARD thought that the tendency to growth of the hair was destroyed chiefly by the chemical action from electricity, or by electrolysis, as was the case in the removal of tumors, although the slight amount of heat unavoidably generated might have some influence.

Dr. MORRILL could not speak from personal experience, but he could testify to the most brilliant and satisfactory results from the method obtained in the hands of Dr. Fox and others; and he believed the operation was destined to become a permanent one.

Dr. JACOBI.—In regard to pulling out the hair, perhaps the plan I have followed in a few instances is one that may prove just as successful, if not a little more so. I do not pull the hair out after the operation, but leave it there; and I have always found, on seeing the patient the next day, if the operation was successful, that the hair was gone. I have done so on purpose, in order to be able to apply the current again when the hair was not removed. When the effect is insufficient, it may be made permanent the next day. With regard to the hyperæmic condition at the insertion of the point, I have not seen that. As a rule, I find immediate anæmia. It is true that reaction sets in afterwards, and I then find some small local swellings.

Dr. W. G. WYLIE, as bearing upon the possible relation between the presence of hair upon the face of the female and the abnormal development of the genitals, said he had noticed a scrotal-like development of the labia in a few cases of bearded women.

It was further stated that successful results had been obtained in cases of scattering hairs by inserting a fine-pointed instrument dipped in some caustic—as the chloride of zinc—into the cavity made after extracting the hair.

The reports of committees were then read.

Dr. ABRAHAM JACOBI read two papers, the first with reference to the establishment of special hospitals for the treatment of scarlet fever and diphtheria within the limits of large cities. The special need of these in the city of New York was discussed quite lengthily by Dr. Woolsey Johnson, member of the Health Board. It was approved by the Society.

The second was with regard to danger to life and limb of factory-children, recommending a new law, embracing the following points: First. Children employed in factories should be under official supervision. In large cities boards of health could be intrusted with it. Second. Before being admitted to factory-work, a child of legal age ought to be examined physically by a medical man. Chlorotic, anæmic, scrofulous, crippled, and phthisical children, and those under normal size for their ages, should be excluded. Third. No night or Sunday work should be permitted. Fourth. Some branches of work should be forbidden entirely, such as mining, glass-works, rag-sorting, working in mercury, lead, arsenic, etc., and in match-factories, and those which are known to interfere with physical development, and others which are known to prove highly dangerous to childhood and adolescence. The earliest age at which the young ought to be admitted to manufacturing employment is fourteen.

This also was approved by the Society.

The Secretary read a communication received from the New York Medico-Chirurgical Society, containing resolutions endorsing the action of the Medical Society of the State of New York in its adoption of the revised system of medical ethics. It was accepted and ordered entered upon the minutes.

The Society then adjourned.

GLEANINGS FROM EXCHANGES.

TWO CASES OF AMPUTATION TREATED WITH EUCALYPTUS GLOBULUS.—Dr. Edward Lawrie, Professor of Surgery in the Lahore Medical School, reports (*Lancet*, January 7) two cases of amputation, one a Carden's amputation of the thigh, and the other, Syme's amputation of the ankle, in which he successfully employed eucalyptus globulus as an antiseptic, both healing without inflammation. His plan of carrying out Listerism with this antiseptic consists in preparing the gauze dressing at the time it is to be used, instead of beforehand. A stock mixture is kept of four to six parts of resin, four parts of spirit, and two parts of castor oil. Carbolic acid, or any other antiseptic, is added to this in the proportion required, and the gauze is impregnated with the mixture at the time of use. In the above instance, as he did not know the proportion of the antiseptic Mr. Lister would employ, the gauze was simply wrung out of the resin mixture and then dipped in tincture of eucalyptus globulus, of which it took up a large quantity very readily. Evaporation was prevented by a mackintosh covering fixed with a splint and bandage; and the only after-treatment consisted in opening this once or twice and moistening the gauze with fresh tincture of eucalyptus. This plan is put forward as an adjunct to Mr. Lister's plan, and not as a rival, its object being to afford a ready method of preparation of the dressings, by which the surgeons may know that they are really antiseptic, the liability of the already-prepared dressings to deteriorate being great, especially in warm climates.

MISCELLANY.

DETECTION OF OLEOMARGARINE.—A process of detecting oleomargarine has been proposed by Messrs. Leune and Harburet. In a communication on this subject to the American Chemical Society by P. Casamajor, the specific gravity of butter is 0.926, which would remain in equilibrium in dilute alcohol of 53.7 per cent., at 15° C. Oleomargarine requiring alcohol of 59.2 per cent. to hold it suspended in the spherical state, the question of the possibility of distinguishing butter from oleomargarine becomes equivalent to the possibility of distinguishing between alcohol of different densities, which may be readily determined, the above difference being represented by 5.5 of Gay-Lussac's alcoholometer. Variations between the two densities are produced by commercial mixtures of oleomargarine and butter: so that several degrees of dilution between the two extremes given above are required in performing this test in the laboratory. As a rule, alcohol of 55 per cent. (and at 15° C.) should be commonly used; and any sample which will not sink to the bottom should be considered oleomargarine. The details of the manipulation are given in an abstract of this paper, published in the *Druggists' Circular and Chemical Gazette* for March, 1882. The proportions of butter and oleomargarine in a mixture may be also determined without the aid of an alcoholometer, by using the two solutions of 53.7 per cent. and 59.2 per cent. These may be placed in graduated glasses and poured cautiously into a third glass, until an alcohol of sufficient strength is obtained to keep a globule of the fat under examination at 15° C. The relative volumes of the two solutions used in making the mixture give the proportions of butter and oleomargarine.

INOCULATION AGAINST ANTHRAX.—Under the auspices of the Royal Hungarian Ministry of Agriculture, Dr. Aladár von Rószahegyi has been testing Pasteur's inoculation process against anthrax, with the following results:

"The results of the experiments performed in Buda-Pesth may be thus summed up. Among the sheep, one died after the first inoculation with catarrhal pneumonia. After the second, one died with gastro-intestinal catarrh. The protective inoculation did not certainly cause any death from anthrax. After the control experiments one animal died of distoma hepaticum, a second of strongylus filaria. Consequently, among the inoculated animals, the virulent control inoculation caused no death which was certainly due to anthrax. There was no death among the uninoculated sheep during the time that the protective experiments were being carried out on the others. After the control inoculation eighty-eight per cent. died of anthrax within eight days, and one (four per cent.) died excessively anæmic. The cattle stood the protective inoculation without loss, and even without any illness. After the control infection there was only some fever of short duration amongst the uninoculated calves. The inoculated calves, as well as all the oxen, were unaffected by the infection."

AN OPEN FIELD FOR SPECIALISTS.—From the *Journal of Comparative Medicine* it is learned that the number of veterinary surgeons in the United States is less than nine hundred, not one-fourth of whom have received a medical degree. The vast moneyed interests at stake conditioned upon the health of various domestic animals are simply immense, and the necessity for more skilled practitioners is actual and urgent. While physicians are in many places poorly remunerated, because the supply is above the demand, competent, well-qualified men have every prospect of success in veterinary medicine, as there are not one-twelfth as many fairly good veterinarians as our commercial and agricultural interests require.

CONTAMINATION BY SEWAGE.—Oysters taken from the beds on the western shore of the Bay of Dublin have been found by Cameron (*Chemical News*, 44 to 52) to be contaminated with sewage. All oysters taken in proximity to large cities should therefore be looked on with suspicion. This circumstance has been repeatedly noticed in the Eastern States, and has already received some attention in the *Review* of February 20, 1880. Klendenin found that pike caught in the vicinity of a sewer contained trichinæ, obviously derived from the consumption of sewage.—*Chicago Medical Review*.

AN ANCIENT LIVER-PAD.—As a rage for foot-pads, kidney-pads, and the pad *genus omne* is now prevailing, a word as to the antiquity is now in order. It is believed that Prometheus was the first victim of a liver-pad. Hamlet probably referred to this incident when he remarked, "Now I could do it pad, while he is preying."

GRAY'S ANATOMY has been translated into the Chinese language.

DR. BOECHAT finds that recent soft goitres yield more rapidly under the external application of iodoform than by the use of either iodine or the iodide of potassium. He uses a glycerite of iodoform which is protected by a coating of collodion.

DR. ARMANGUE treats tonsillitis by hot local application, several times per day, of bicarbonate of soda. But we believe that aconite is a specific for tonsillitis, and should be loath to use anything else.—*Physician and Surgeon.*

M. CHARCOT has been appointed to the newly-created chair of Clinical Professor of Diseases of the Nervous System in the Faculty of Medicine, Paris.

PROF. ERNST HAECKEL, the well-known German biologist, is engaged in a scientific mission in Ceylon, after completing which he will proceed to India.

THE death of Sir Robert Christison is announced. He was in his eighty-fifth year.

DR. THIN (*Practitioner*) uses a solution of boracic acid in glycerin made into a cream with wax and almond oil for fetid sweating of the feet.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY FROM FEBRUARY 19 TO MARCH 4, 1882.

POPE, BENJ. F., CAPTAIN AND ASSISTANT-SURGEON.—Relieved from duty in Department of Dakota, and to report in person to the Surgeon-General for duty in his office. S. O. 42, A. G. O., February 21, 1882.

MOFFATT, PETER, CAPTAIN AND ASSISTANT-SURGEON.—Relieved from duty at Camp Spokane, W.T., to proceed to Fort Cœur d'Alène, Idaho, and relieve Assistant-Surgeon Spencer from duty at that post. S. O. 20, Department of the Columbia, February 11, 1882.

FINLEY, J. A., CAPTAIN AND ASSISTANT-SURGEON, FORT ADAMS, R.I.—Granted leave of absence for one month. S. O. 29, Department of the East, February 20, 1882.

COMEGYS, E. T., CAPTAIN AND ASSISTANT-SURGEON.—Assigned to duty at Fort Stanton, N. Mex., relieving Assistant-Surgeon Newton. S. O. 40, Department of the Missouri, February 21, 1882.

SPENCER, WM. G., CAPTAIN AND ASSISTANT-SURGEON.—Granted leave of absence for one month, with permission to apply for an extension of one month to Division Headquarters, and for an extension of four months to the Adjutant-General of the Army, S. O. 19, Department of the Columbia, February 10, 1882, and relieved from duty in Department of the Columbia, to proceed to New York City, and, on arrival, report by letter to the Surgeon-General. S. O. 46, A. G. O., February 27, 1882.

POWELL, J. L., FIRST-LIEUTENANT AND ASSISTANT-SURGEON, FORT STOCKTON, TEXAS.—Granted leave of absence for one month. S. O. 19, Department of Texas, February 24, 1882.

NEWTON, R. C., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—When relieved by Assistant-Surgeon Comegys, to proceed to Fort Cummings, N. Mex., and report to the Commanding Officer for duty. S. O. 40, c. s., Department of the Missouri.

RAYMOND, H. J., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Relieved from duty at the Presidio of San Francisco, and assigned to duty at Alcatraz Island, California. S. O. 28, Military Division of the Pacific and Department of California, February 15, 1882.

FRANTZ, JOHN H., MAJOR AND SURGEON.—Died at Baltimore, Md., on March 2, 1882.